3,

PROCEEDINGS

OF

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FOR THE

ADVANCEMENT OF SCIENCE,

THIRTY-SEVENTH MEETING,

HELD AT

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TABLE OF CONTENTS.

IN GENERAL SESSIONS.

SECTION A. MATHEMATICS AND ASTRONOMY.

SECTION B. PHYSICS.

SECTION C. CHEMISTRY.

SECTION D. MECHANICAL SCIENCE AND ENGINEERING.

SECTION E. GEOLOGY AND GEOGRAPHY.

SECTION F. BIOLOGY.

SECTION H. ANTHROPOLOGY.

SECTION I. ECONOMIC SCIENCE AND STATISTICS.

CLUBS OF A. A. A. S.

EXECUTIVE PROCEEDINGS.

INDEX.

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M. E. Rawson, W. H. Searles, Wm. E. Sherwood.

COMMITTEE ON THE PRESS.

Chairman: PROF. BERNADOTTE PERRIN.
Secretary: PROF. A. H. THOMPSON.

Prof. A. H. Palmer, Prof. C. E. Williams, W. E. Cushing, J. C. Covert, J. B. Morrow, J. H. A. Bone, N. S. Cobleigh, R. F. Paine, jr., C. B. Williams, J. C. Keffer, Geo. A. Robertson, W. Scott Robinson, Chas. P. Salen, E. W. Doty, Maj. Wm. Kaufman, C. L. Hotze, Carl Claussen.

COMMITTEE ON MEMBERSHIP.

Chairman: HON. C. C. BALDWIN.
Secretary: REV. JABEZ HALL.

Rev. Dr. M. Machol, Miss Mary Evans, Harry P. Cushing.

COUNCILMANIC COMMITTEE.

Councilman J. T. Logue, Councilman H. Schnauffer, Alderman P. J. Cooney.

COMMITTEE ON DECORATION.

Mrs. William Bingham, Miss K. Mather, Miss Julia Parsons, Mrs. C. H. Bulkley, Mrs. C. A. Brayton, Mrs. D. R. Tilden.

COMMITTEE ON LUNCH.

Mrs. P. H. Babcock, Mrs. S. T. Wellman, Mrs. Julius King.

SPECIAL COMMITTEES OF THE ASSOCIATION.[1]

1. *Committee on Indexing Chemical Literature.*

H. CARRINGTON BOLTON of Hartford,
F. W. CLARK of Washington,
A. R. LEEDS of Hoboken,
A. A. JULIEN of New York,
J. W. LANGLEY of Ann Arbor,
C. K. WEAD of Malone.
A. B. PRESCOTT of Ann Arbor.

2. *Committee on International Congress of Geologists.*

JAMES HALL of Albany,
J. W. DAWSON of Montreal,
J. S. NEWBERRY of New York,
T. STERRY HUNT of Montreal,
C. H. HITCHCOCK of Hanover,
RAPHAEL PUMPELLY of Newport,
J. P. LESLEY of Philadelphia,
J. W. POWELL of Washington,
G. A. COOK of New Brunswick,
JOHN J. STEVENSON of New York,
E. D. COPE of Philadelphia,
E. A. SMITH of Tuscaloosa,
PERSIFOR FRAZER of Philadelphia,
H. S. WILLIAMS of Ithaca,
N. H. WINCHELL of Minneapolis.

3. *Committee on Anatomical Nomenclature, with Special reference to the Brain.*

HARRISON ALLEN of Philadelphia,
H. F. OSBORN of Garrisons,
T. B. STOWELL of Cortland,
B. G. WILDER of Ithaca,
FRANK BAKER of Washington.

4. *Committee on Physics Teaching.*

T. C. MENDENHALL of Terre Haute,
W. A. ANTHONY of Manchester,
H. S. CARHART of Ann Arbor,
F. H. SMITH of Charlottesville,
JOHN TROWBRIDGE of Cambridge.

5. *Committee to apply to Congress for a Reduction of the Tariff on Scientific Books and Apparatus.*

E. D. COPE of Philadelphia,
J. R. EASTMAN of Washington,
JOHN S. BILLINGS of Washington.

[1] All Committees are expected to present their reports to the COUNCIL not later than the fourth day of the meeting. Committees sending their reports to the Permanent Secretary one month before a meeting can have them printed for use at the meeting.

6. *Committee to memorialize Congress to take steps for the Preservation of Archæologic Monuments on the public lands.*

MISS ALICE C. FLETCHER of Cambridge, | MRS. T. E. STEVENSON of Washington.

7. *Committee on Universal Language.*

HORATIO HALE of Clinton, | H. W. HENSHAW of Washington,
ALEX. MACFARLANE of Austin.

8. *Committee on Chemistry Teaching.*

W. H. SEAMAN of Clinton, | H. W. WILEY of Washington,
W. L. DUDLEY of Nashville, | W. O. ATWATER of Middletown,
W. A. NOYES of Terre Haute.

9. *Committee on Water Analysis.*

G. C. CALDWELL of Ithaca, | J. W. LANGLEY of Ann Arbor,
J. A. MYERS of Agricultural Coll., Miss., | W. P. MASON of Troy,
R. B. WARDER of Washington, | W. H. SEAMAN of Washington.

10. *Committee of conference on organization of a National Chemical Society.*

A. P. PRESCOTT of Ann Arbor, | ALFRED SPRINGER of Cincinnati,
EDWARD HART of Easton.

11. *Special Agent of Transportation for the Association to act with the Local Committees.*

P. H. DUDLEY of New York.

12. *Auditors.*

HENRY WHEATLAND of Salem, | THOMAS MEEHAN of Germantown.

OFFICERS ELECTED

FOR THE

TORONTO MEETING.

PRESIDENT.

THOMAS C. MENDENHALL of Terre Haute, Ind.

VICE PRESIDENTS.

A. Mathematics and Astronomy—ROBERT S. WOODWARD of Washington, D. C.
B. Physics—HENRY S. CARHART of Ann Arbor, Mich.
C. Chemistry—WILLIAM L. DUDLEY of Nashville, Tenn.
D. Mechanical Science and Engineering—ARTHUR BEARDSLEY of Swarthmore, Pa.
E. Geology and Geography—CHARLES A. WHITE of Washington.
F. Biology—GEORGE L. GOODALE of Cambridge, Mass.
H. Anthropology—GARRICK MALLERY of Washington.
I. Economic Science and Statistics—CHARLES S. HILL of Washington.

PERMANENT SECRETARY.

FREDERICK W. PUTNAM of Cambridge, Mass. (office Salem, Mass.).

GENERAL SECRETARY.

C. LEO MEES of Terre Haute, Ind.

SECRETARY OF THE COUNCIL.

FRANK BAKER of Washington.

SECRETARIES OF THE SECTIONS.

A. Mathematics and Astronomy—GEORGE C. COMSTOCK of Madison, Wis.
B. Physics—EDWARD L. NICHOLS of Ithaca, N. Y.
C. Chemistry—EDWARD HART of Easton, Pa.
D. Mechanical Science and Engineering—JAMES E. DENTON of Hoboken, N. J.
E. Geology and Geography—JOHN C. BRANNER of Little Rock, Ark.
F. Biology—AMOS W. BUTLER of Brookville, Ind.
H. Anthropology—WILLIAM M. BEAUCHAMP of Baldwinsville, N. J.
I. Economic Science and Statistics—JOHN R. DODGE of Washington, D. C.

TREASURER.

WILLIAM LILLY of Mauch Chunk, Pa.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIS'T SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, Jr.,* C. B. Trego,*	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*	J. D. Whitney, M. B. Williams,*	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, Jr.,*		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, Jr.,* O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,*	B. Silliman, Jr.,* J. Lawrence Smith,*		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, Jr.,*		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†*	Jeffries Wyman,*		B. Silliman, Jr.*

* Deceased.

† Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.*
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.*
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5*	S. F. Baird,*	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,*	S. F. Baird,*	A. L. Elwyn.*
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		S. St. John,* 7	S. F. Baird,*	A. L. Elwyn.*
8th	April 26, 1854,	Washington, D. C.,	J. D. Dana,		J. Lawrence Smith,*	Joseph Lovering,	J. L. LeConte* 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.*
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.*
11th	Aug. 12, 1857,	Montreal, Canada,	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.*
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.*
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.*
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,*	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.*
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,*	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†*
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†*
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,*	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†*
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†*
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†*
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,*	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.*
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,*	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.*
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†*	C. A. White,	F. W. Putnam,	W. S. Vaux.*
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,*	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†*

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,*	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.*
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.*
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, jr.,	———	W. S. Vaux.†*
F. W. Putnam,	H. C. Bolton,	F. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	G. J. Engelmann,	———	W. S. Vaux.†*
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,*	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†*
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.*
F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†*

[1] In the absence of E. C. Pickering.
[4] In the absence of Ira Remsen.
[7] In the absence of A. M. Mayer.
[10] In the absence of John Trowbridge.
* Deceased.

[2] The Subsection united with Sec. B.
[5] In the absence of George Little.
[8] In the absence of George Engelmann.
[11] In the absence of S. P. Sharples.
† Not present.

[3] In the absence of G. S. Blackie.
[6] In the absence of A. C. Wetherby.
[9] In the absence of W. R. Nichols.
[12] In place of H. W. Wiley, called away.

MEETINGS AND OFFICERS OF THE ASSOCIATION. (*Continued.*)

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENTS. Section A.	Section B.	Section C.	Section D.	Section E.
31st	Aug. 23, 1882.	Montreal, Can.	J. W. Dawson,	W. A. Rogers,[1]	T.C. Mendenhall,	H. C. Bolton,	W. P. Trowbridge,	E. T. Cox,
32nd	Aug. 15, 1883.	Minneapolis, M.	C. A. Young,	W. A. Rogers,	H. A. Rowland,	E. W. Morley,	De Volsen Wood,[7]	C.H. Hitchcock,
33rd	Sept. 3, 1884.	Philadelphia, Pa.	J. P. Lesley.	H. T. Eddy,	J. Trowbridge.	J. W. Langley,	R. H. Thurston,	N. H. Winchell,
34th	Aug. 26, 1885.	Ann Arbor, Mich.	H. A. Newton,	W. Harkness,[15]	S. P. Langley,[16]	N. T. Lupton,[17]	J. Burkitt Webb,	Edward Orton,
35th	Aug. 18, 1886.	Buffalo, N. Y.	E. S. Morse,	J. W. Gibbs,	C. F. Brackett,	H. W. Wiley,	O Chanute,	T. C. Chamberlin,
36th	Aug. 10, 1887.	New York,	S. P. Langley,	J. R. Eastman,[26]	W. A. Anthony,	A. B. Prescott	E. B. Coxe,	G. K. Gilbert,

VICE PRESIDENTS. Section F.	Section G.	Section H.	Section I.	Permanent Secretary.	General Secretary.	Assistant Gen. Secretary.	SECRETARIES OF SECTIONS. Section A.	Section B.
W. H. Dall,	A. H. Tuttle,	Alex. Winchell,[2]	E. B. Elliott,	F. W. Putnam,	Wm. Saunders,	J. R. Eastman.	H. T. Eddy,	C. S. Hastings,
W. J. Beal,	J. D. Cox.	O. T. Mason,	F. B. Hough,*	F. W. Putnam,	J. R. Eastman.	Alfred Springer,	G. W. Hough,[8]	F. E. Nipher,[9]
E. D. Cope.	T. G. Wormley,	E. S. Morse,	John Eaton,	F. W. Putnam,	Alfred Springer,	C. S. Minot.[12]	G. W. Hough,	N. D. C. Hodges,
T. J. Burrill,[18]	S. H. Gage,	J. O. Dorsey,[19]	Edw. Atkinson	F. W. Putnam,	C. S. Minot,	S. G. Williams,[20]	E. W. Hyde.	B. F. Thomas,[21]
H. P. Bowditch,	——————[25]	H. Hale,	Jos. Cummings	F. W. Putnam,	S. G. Williams,	W. H. Pettee,	S. C. Chandler,	H. S. Carhart,
W. G. Farlow,	——————	D. G. Brinton,	H. E. Alvord,	F. W. Putnam.	W. H. Pettee,	J. C. Arthur,	H. M. Paul,	C. Leo Mees,

SECRETARIES OF THE SECTIONS. Section C.	Section D.	Section E.	Section F.	Section G.	Section H.	Section I.	Treasurer.
Alfred Springer,	J. B. Webb,[3]	H. S. Williams,[4]	Wm. Osler,[5]	Robt. Brown, jr.,	O. T. Mason,	F. B. Hough,[6]*	——————*
J. W. Langley,[10]	J. B. Webb,[11]	A. A. Julien,	S. A. Forbes,	Carl Seiler,	G. H. Perkins,	Jos. Cummings,	William Lilly.
H. Carmichael,[13]	J. B. Webb,	E. A. Smith,	C. E. Bessey,	R. Hitchcock,	G. H. Perkins,[14]	C. W. Smiley,	William Lilly.
F.P. Dunnington,	C. J. H. Woodbury,	G. K. Gilbert,[22]	J. A. Lintner,[23]	W. H. Walmsley.	Erm. A. Smith,*	C. W. Smiley,[24]	William Lilly.
W. McMurtrie,	William Kent,	E. W. Claypole,	J. C. Arthur,	——————[25]	A. W. Butler,	H. E. Alvord,	William Lilly.
C. S. Mabery,[27]	G. M. Bond,	W. M. Davis,[28]	J. H. Comstock,	——————	C. C. Abbott,[29]	W. R. Lazenby,	William Lilly.

1 In the absence of William Harkness.
2 In the absence of Daniel Wilson.
3 In the absence of C. B. Dudley.
4 In the absence of O. E. Dutton.
5 In the absence of C. S. Minot.
6 F. B. Hough was called home and J. R. Dodge acted at the close of the meeting.
7 Absent but place was not filled.
8 In the absence of W. W. Johnson.
9 In the absence of C. K. Wead.
10 W. M. McMurtrie, last three days of meeting.
11 Pro tempore.
12 In the absence of E. S. Holden.
13 In the absence of R. B. Warder.
14 In the absence of W. H. Holmes.
15 In the absence of J. M. Van Vleck.
16 In place of C. F. Brackett, resigned.
17 In the absence of W. R. Nichols.
18 In the absence of B. G. Wilder.
19 In the absence of W. H. Dall.
20 In place of C. C. Abbott, not present
21 In place of A. A. Michelson, resigned.
22 In the absence of H. C. Lewis.
23 In place of C. H. Fernald, resigned.
24 In the absence of J. W. Chickering.
25 Section permanently united to Section F.
26 In place of W. Ferrel, resigned.
27 Absent; R. B. Warder acted as Sec'y pro tem.
28 In place of T. B. Comstock, absent.
29 In place of F. W. Langdon, absent.

*Deceased.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.
37th.	Aug. 15, 1888.	Cleveland, Ohio.	J. W. Powell.

VICE PRESIDENTS.

SECTION A.	SECTION B.	SECTION C.	SECTION D.
Ormond Stone.	A. A. Michelson.	C. E. Munroe.	C. J. H. Woodbury.

SECTION E.	SECTION F.	SECTION H.	SECTION I.
George H. Cook.	C. V. Riley.	C. C. Abbott.	C. W. Smiley.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF COUNCIL.	TREASURER.
F. W. Putnam.	Julius Pohlman.	C. Leo Mees.	William Lilly.

SECRETARIES OF SECTIONS.

SECTION A.	SECTION B.	SECTION C.	SECTION D.
C. C. Doolittle.	A. Macfarlane.	W. L. Dudley.	Arthur Beardsley.

SECTION E.	SECTION F.	SECTION H.	SECTION I.
John C. Branner.	B. H. Fernow.	Frank Baker.	Charles S. Hill.

MEETINGS.	PLACE.	YEAR.	MEMBERS IN ATTENDANCE.	NUMBER OF MEMBERS.
1.	Philadelphia	1848	?	461
2.	Cambridge	1849	?	540
3.	Charleston	1850	?	622
4.	New Haven	1850	?	704
5.	Cincinnati	1851	87	800
6.	Albany	1851	194	769
7.	Cleveland	1853	?	940
8.	Washington	1854	168	1004
9.	Providence	1855	166	605
10.	2nd Albany	1856	381	722
11.	Montreal	1857	351	946
12.	Baltimore	1858	190	962.
13.	Springfield	1859	190	862
14.	Newport	1860	135	644
15.	Buffalo	1866	79	637
16.	Burlington	1867	73	415
17.	Chicago	1868	259	686
18.	Salem	1869	244	511
19.	Troy	1870	188	536
20.	Indianapolis	1871	196	668
21.	Dubuque	1872	164	610
22.	Portland	1873	195	670
23.	Hartford	1874	224	722
24.	Detroit	1875	165	807
25.	2nd Buffalo	1876	215	867
26.	Nashville	1877	173	953
27.	St. Louis	1878	134	962
28.	Saratoga	1879	256	1030
29.	Boston	1880	997	1555
30.	2nd Cincinnati	1881	500	1699
31.	2nd Montreal	1882	937	1922
32.	Minneapolis	1883	328	2033
33.	2nd Philadelphia	1884	1261*	1981
34.	Ann Arbor	1885	364	1956
35.	3d Buffalo	1886	445	1886
36.	New York	1887	729	1956
37.	2nd Cleveland	1888	342	1964
38.	Toronto	1889	—	—

*Including members of the British Association and other foreign guests.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be, possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, and election by the Council.

ART. 4. Fellows shall be elected by the Council from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members of the Council at a designated meeting of the Council.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Council and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privi-

leges of Fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Council shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Council made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, a Secretary of the Council, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Council. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Council shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of

these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting.

ART. 13. The Secretary of the Council shall keep the records of the Council. He shall give to the Secretary of each Section the titles of papers assigned to it by the Council. He shall receive proposals for membership and bring them before the Council.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Council. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Council, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Council, and shall pay over to the Treasurer such unexpended funds as the Council may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Council. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Council, and may employ one or more clerks at such compensation as may be agreed upon by the Council.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Council. He shall annually present to the Council an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Council, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Council.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Council as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Secretary of the Council, and Treasurer, shall be filled by nomination of the Council and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Council shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Secretary of the Council, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Council, provided there are at least five, shall form a quorum for the transaction of business. The Council shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Council shall be held in the council room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Council may be called at any time by the President. The Council shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Council. The Council shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers,

discussions and other proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; elect members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association. The Council shall receive all reports of Special Committees and decide upon them, and only such shall be read in General Session as the Council shall direct. The Council shall appoint at each meeting the following sub-committees who shall act, subject to appeal to the whole Council, until their successors are appointed at the following meeting: 1, on Papers and Reports; 2, on Members; 3, on Fellows.

ART. 19. The Nominating Committee shall consist of the Council, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Council may designate.

ART. 21. A General Session shall be held at 10 o'clock A. M., on the first day of the meeting, and at such other times as the Council may direct.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into Sections, namely:—A, *Mathematics and Astronomy*; B, *Physics*; C, *Chemistry, including its application to agriculture and the arts*; D, *Mechanical Science and Engineering*; E, *Geology and Geography*; F, *Biology*; [G, united to section F]; H, *Anthropology*; I, *Economic Science and Statistics*. The Council shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section or Subsection until it has been received from the Council and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Council. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Council with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Council to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

ART. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press, and these must be in the hands of the Secretaries of the Sections before the final adjournment of the meeting, otherwise only the titles will appear in the printed volume. The Council shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Council. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Council. The Council shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets

in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Council.

Admission Fee and Assessments.

Art. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 35. The annual assessment for members and fellows shall be three dollars.

Art. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which, during the life of the member, shall form a part of the general fund of the Association; but, after his death, shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Council.

Art. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Council.

Alterations of the Constitution.

Art. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Announcements of Elections by the Council.
7. Unenumerated business.
8. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28) **F E**
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

MEMBERS.[3]

Abbe, Robert, 11 W. 50th St., New York, N. Y. (36).
Abert, S. Thayer, 810 19th St., N. W., Washington, D. C. (30). **A B D E I**
Adams, W. H., Consulting Engineer, 71 Wall St., New York, N. Y. (36).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Aher, Mrs. Mary R. Alling, Waterbury, Conn. (29). **E F C**
Alberger, Louis R., P. O. Box 2227, New York, N. Y. (35). **C**
Alden, Jno., Pacific Mills, Lawrence, Mass. (36).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The black letters at the end of line indicate the sections to which members elect to belong. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes to which he may be entitled, bound in cloth, or by the payment of twenty dollars, to such volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.
The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The income of the money derived from a Life Membership is used for the general purposes of the Association during the life of the member; afterwards it is to be used to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allderdice, Wm. H., U. S. S. Lancaster, care B. F. Stevens, 4 Trafalgar Square, Charing Cross, London, Eng. (33). D
Allen, Addison, 50 Wall St., New York, N. Y. (36).
Allen, Dudley P., 177 Euclid Ave., Cleveland, Ohio (36) F
Allen, J. M., Hartford, Conn. (22). D
Allen, W. F., 46 Bond St., New York, N. Y. (36).
Alvord, Benjamin, 2nd Lt., U. S. A., West Point, N. Y. (33). A
Ammidown, Edward H., P. O. Box 2739, New York, N. Y. (37).
Anderson, Newton M., 371 Sibley St., Cleveland, Ohio. (30). B
Angell, Geo. W. J., 44 Hudson St., New York, N. Y. (36).
Ansley, Clark F., Swedona, Mercer Co., Ill. (32). E H
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Archambault, U. E., P. O. box 1944, Montreal, Can. (31).
Arey, Albert L., Free Academy, Rochester, N. Y. (35). B C
Arms, Walter F., Punxsutawney, Jefferson Co., Pa. (35).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26). D
Atterbury, Rev. Anson Phelps, 117 W. 87th St., New York, N. Y. (36). E
Atwell, Charles B., 461 Emerson St., Evanston, Ill. (36). F
Atwood, E. S., East Orange, N. J. (29). F
Austin, Wm., 65 Union Place, New York, N. Y. (36).
Avery, Elroy, M., Ph. D., Woodland Hills Ave.. Cleveland, Ohio (37). B
AVERY, SAMUEL P., 4 E. 38th St., New York, N. Y. (36).
Ayer, Edward Everett, 234, S. Water St., Chicago, Ill. (37). H

Baba, Tatui, 126 W. 42nd St., New York, N. Y. (36). H
Babcock, Geo. H., 30 Cortlandt St., New York, N. Y. (33). D
Bacon, Prof. Chas. A., Observatory Beloit College, Beloit, Wis. (36) A
Bailey, E. H. S., Lawrence, Douglas Co., Kan. (25). C E
Baker, Richard D., 1414 Arch St., Philadelphia, Pa. (33). E C
Baker, Wm. G., 234 E. 15th St., New York, N. Y. (36).
Balderston, C. Canby, Westtown, Chester Co., Pa. (33). B
Baldwin, Judge Charles C., 1264 Euclid Ave., Cleveland, Ohio (37). H I
Baldwin, Miss Mary A., 41 Fulton St., Newark, N. J. (31). E
Baldwin, Mrs. G. H., 3 Madison Ave., Detroit, Mich. (34). H
Ballard, Harlan H., 50 South St., Pittsfield, Mass. (31). E F
Balliard, Chas., Metropolitan Museum of Art, New York, N. Y. (36).
Banes, Charles H., 2021 Spring Garden St., Philadelphia, Pa. (31). D
BANGS, LEMUEL BOLTON, M.D., 127 E. 34th St., New York, N. Y. (36).
Barber, D. H., Marion, Iowa (37).
Barber, John N., 1317 Hennepin Ave., Minneapolis, Minn. (33). F
Barclay, Robert, A.M., M.D., 3211 Lucas Ave., St. Louis, Mo. (30).
Bardeen, Francis L., M.D., Box 76, Onondaga Hill, N. Y. (32). C F B
BARGE, B. F., Mauch Chunk, Pa. (33).
Barker, Mrs. Martha M., 26 Eleventh St., Lowell, Mass. (31). E H
Barnett, J. Davis, Port Hope, Ontario, Can. (34). D B
Barnum, Miss Charlotte C., 144 Humphrey St., New Haven, Conn. (36). A
Barnum, Thomas R., 144 Humphrey St., New Haven, Conn. (35). E

Barrett, Dwight H., B. & O. R. R. Co., Baltimore, Md. (36).
Bartlett, John W., M.D., 149 W. 94th St., New York, N. Y. (36).
Bartley, Elias H., M.D., 21 Lafayette Ave., Brooklyn, N. Y. (33). C
Bassett, Norman C., Mechanical Engineer, Hale Elevator Co., Room 81, 187 La Salle St., Chicago. Ill. (35). D
Battershall, Jesse Park, 402 Washington St., New York, N. Y. (36).
Battle, Herbert B., N. C. Agric. Exper. Station, Raleigh, N. C. (33). C
Baur, George, New Haven, Conn. (36).
Baxter, James N., 18 Fulton St., Brooklyn, N. Y. (36).
Baxter, Sylvester, office of the Herald, Boston, Mass. (36). H
Beach, William H., Madison, Wis. (21). E B
Bean, Thos. E., Box 441, Galena, Ill. (28). F
Beardslee, H. C., 801 Wilson Ave., Cleveland, Ohio. (37). F
Bechdolt, Adolphus F., Supt. City Schools, Mankato, Minn. (32). E B F
Becker, Dr. Geo. F., U. S. Geol. Survey, San Francisco, Cal. (36). E
Belknap, Morris B., Louisville, Ky. (29). H E
Belknap, Wm. B., Louisville, Ky. (29). D
BELL, C. M., M.D., 320 Fifth Ave., New York, N. Y. (36).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19). B C
Bennett, Prof. Wm. Z., Wooster, Wayne Co., Ohio (33). C
Beveridge, David, 145 Griswold St., Detroit, Mich. (33). I
Bien, Julius, 139 Duane St., New York, N. Y. (34). E H
Bigelow, Otis, 605 7th St., Washington, D. C. (30). H F
Bigelow, Robert P., Washington, D. C. (32). F E
Bill, Charles, Springfield, Mass. (17). H F I
Bingham, Mrs. Martha A., Hotel Brunswick, Kansas City, Mo. (32).
Birdsall, Miss Louise W., 105 E. 37th St., New York, N. Y. (37). F E
Birge, Charles P., Keokuk, Iowa (29). E
BISHOP, HEBER R., Mills Building, New York, N. Y. (36).
Bishop, Irving P., Chatham, Columbia Co., N. Y. (35). E
BIXBY, WM. H., Wilmington, N. C. (34). D
Blacklock, Charles H., Rugby, Tenn. (35).
Blackwell, Mrs. A. B., Elizabeth, N. J. (30). F C B
Blaisdell, F. E., Coronado, San Diego Co., Cal. (29). F
Blake, Francis C., Mansfield Valley P. O., Allegheny Co., Pa. (29). C B D
Blake, L. I., Hyde Park, Mass. (36).
Blakslee, Prof. T. M., Des Moines, Iowa (31). A
Blatchford, Eliphalet W., 375 La Salle Ave., Chicago, Ill. (17). F
Bleile, Albert M., M.D., 342 S. Fourth St., Columbus, Ohio. (37) F
BLISH, W. G., Niles, Mich. (33). B D
Blount, Henry F., Evansville, Vanderburg Co., Ind. (32). I B
Blount, Mrs. Lucia E., 518 Unity Square, Kalamazoo, Mich. (34). H I
Blue, Archibald, Ass't Minister of Agric., Toronto, Can. (35). I
Bohannan, R. W., University of Virginia, Va. (36).
Bolton, Prof. John, 49 Huntington St., Cleveland, Ohio. (37).
Booraem, J. V. V., 204 Lincoln Place, Brooklyn, N. Y. (36).
Booth, Miss Mary A., Longmeadow, Mass. (34). F I
Booth, Samuel C., Longmeadow, Mass. (34). E I

Bothwell, Geo. W., M.A., Oakland, Cal. (36).
Bourland, Addison M., M.D., Van Buren, Ark. (29). C E F
Bourne, Robert W., Box 217, Providence, R. I. (34). D
Bowers, Miss Virginia K., 61 Taylor St., Newport, Ky. (27). F H B C
Bowman, Chas. G., Lt. U. S. N., care Navy Dept., Washington, D.C. (33)
Bowser, Mrs. Anna C., 828 Third St., Louisville, Ky. (33). H
Boyer, Jerome L., Sup't Chestnut Hill Iron Ore Co., Reading, Pa. (35). D
Brackett, Richard N., Chemist of the Geological Survey of Arkansas, Little Rock, Ark. (37). C E
Bradford, Edward H., M.D., 150 Boylston St., Boston, Mass. (29).
Bradford, Royal B., Lt. Comd'r, U. S. N., care of Navy Department, Washington, D. C. (31). B D
Braid, Andrew, U. S. Coast and Geodetic Survey Office, Washington, D. C. (33). C B A
Braid, James W., Nashville, Tenn. (33).
Bray, Prof. C. D., College Hill, Mass. (29). D B
Brayton, Miss Sarah H., M.D., Evanston, Ill. (33).
Breckinridge, S. M., P. O. Box 346, St. Louis, Mo. (27).
Breidenbaugh, Prof. E. S., Pennsylvania College, Gettysburg, Pa. (33). C
Brice, Judge Albert G., 122 Gravier St., New Orleans, La. (32). H
Brill, Prof. Charles C., Northfield, Vt. (36). F
Bringhurst, Prof. W. L., Agric. and Mechan. College of Texas, College Station, Brazos Co., Texas (32).
Bristol, Wm. H., Stevens Institute, Hoboken, N. Y. (36). A B D
Britton, Mrs. N. L., Columbia College, New York, N. Y. (31). F
Bromfield, Rev. Edw. T., Glenbrook, Fairfield Co., Conn. (33). F H
Brooke, Dr. Emma W., 15th and Chestnut Sts., Phila., Pa. (33). C E
Broomall, Hon. John M., Media, Delaware Co., Pa. (23). A
Brown, Albert P., Ph.D., 501 Federal St., Camden, N. J. (33). F C
Brown, Miss Anna M., 528 West 7th St., Cincinnati, Ohio (31). F
Brown, Prof. C. J., Clark Univ., Atlanta, Ga. (31). C B
Brown, Fred. G., Fort Ann, Washington Co., N. Y. (31). C E
Brown, Rev. Henry M., East Aurora, Erie Co., N. Y. (35). F C
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brown, Paul Taylor, 2206 Green St., Philadelphia, Pa. (33). E
Brownell, Silas B., 71 Wall St., New York, N. Y. (36).
Brownell, Prof. Walter A., 125 Univ. Ave., Syracuse, N. Y. (30). E B C
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Waring, John, care Mather Electric Co., Hartford, Conn. (33). D B
Warner, Hulbert H., Rochester, N. Y. (31). A
Warner, Worcester R., 887 Case Ave., Cleveland, Ohio (33). A B D
Warren, A. Coolidge, 239 W. 21st St., The Delaware, New York, N. Y. (34). E H
Warren, Eugene C., 611 W. Main St., Louisville, Ky. (37).
Warren, Miss Lillie E., 239 W. 21st St., The Delaware, New York, N. Y. (35). H E
Warren, Mrs. Susan E., 67 Mt. Vernon St., Boston, Mass. (29).
Warrington, James N., Vulcan Iron Works, 86 No. Clinton St., Chicago, Ill. (34). D A B
Waterhouse, A., M.D., 42 Allen St., Jamestown, N. Y. (29). F
Waters, Edwin F., 131 Newbury St., Boston, Mass. (29).
WATERS, GEO. F., 8 Beacon St., Boston, Mass. (29). B F H E D
Watkins, L. D., Manchester, Mich. (34). C
Watson, Miss C. A., Salem, Mass. (31). D
Webb, Dr. DeWitt, St. George St., St. Augustine, Fla. (36).
Webb, Miss Sarah E., Elizabeth, N. J. (33). H F
Webster, F. M., La Fayette, Ind. (35).
Weed, J. N., Newburgh, N. Y. (37). E I
Weeden, Hon. Joseph E., Randolph, N. Y. (31).
Weeks, Joseph D., Editor American Manufacturer, Pittsburgh, Pa. (35). D
Weitbrecht, George, 469 North St., St. Paul, Minn. (32). C E
Welch, Thomas V., Sup't State Reservation, Niagara Falls, N. Y. (35).
Wells, Mrs. C. F., 775 Broadway, New York, N. Y. (31). H F I D B
Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24). H
Wendte, Rev. C. W., Oakland, Cal. (29). H I
West, Mrs. E. S., 54 W. 55th St., New York, N. Y. (36). E
Westbrook, Benj. F., M.D., 174 Clinton St., Brooklyn, N. Y. (33).
Wetzler, Jos., Room 175, Potter Building, New York, N. Y. (36).
Wheeler, Arthur S., Tulane Univ., New Orleans, La. (36).
Wheeler, Herbert A., Washington Univ., St. Louis, Mo. (33). E I
Wheeler, Moses D., M.E., P. O. Box 539, Stapleton, Staten Island, N. Y. (35). D
Wheeler, T. B., M.D., 2344 St. Catherine St., Montreal, Canada (11).
Wheildon, Miss Alice W., Concord, Mass. (31).
Whelen, Edw. S., 1520 Walnut St., Philadelphia, Pa. (33).
Whetstone, John L., Summit Ave., Mt. Auburn, Cincinnati, Ohio (30). D

White, Charles H., Surgeon U. S. N.; Mus. of Hygiene, 1709 N. Y. Ave., N. W., Washington, D. C. (34). C
White, Prof. Charles Joyce, Cambridge, Mass. (29). A
White, James G., Univ. of Neb., Lincoln, Neb. (34). B A
White, John Fleming (35). C
White, LeRoy S., Box 324, Waterbury, Conn. (28).
White, Loomis L., 7 E. 44th St., New York, N. Y. (36).
White, Z. L., Washington, D. C. (37). I
Whiting, S. B., Pottsville, Pa. (33). D
Whitlock, Prof. Roger H., Coll. Station, Brazos Co., Texas (36). A B D
Wiechmann, F. G., School of Mines, Columbia College, New York, N. Y. (30). C E
Wight, Orlando W., M.D., Detroit, Mich. (34). H F
Wilbour, Mrs. Charlotte B., care J. M. Furley, with Haley & Co., 397 Broadway, New York, N. Y. (28).
Wilcox, Miss Emily T., 85 Second St., Troy, N. Y. (33). B A
Wilder, Alex., M.D., 565 Orange St., Newark, N. J. (29). H F I
Wilkinson, J. Henderson, 320 E. Capitol St., Washington, D. C. (35). E
Wilkinson, Mrs. L. V., Seventy Six P. O., Perry Co., Mo. (30).
Willet, Joseph Edgerton, Macon, Ga. (36). E
Willetts, Joseph C., Skaneateles, N. Y. (29). E F H
Williams, C. T., 871 Case Ave., Cleveland, Ohio (37).
Williams, Prof. Edward H., jr., Box 463, Bethlehem, Pa. (25). E D
Williams, Francis H., M.D., Hotel Victoria, Boston, Mass. (29).
Williams, H. Smith, M.D., Independence, Iowa (34). F
Williams, J. Francis, Salem, N. Y. (31). C E
Willis, Bailey, U. S. Geol. Survey, Washington, D. C. (36).
Willson, Robert W., Cambridge, Mass. (30). B A
Wilmot, Thos. J., Commercial Cable Co., Waterville, County Kerry, Ireland (27). B
Wilson, C. H., Belize, British Honduras (30). E C D
Wilson, Chas. M., M.D., 1517 Walnut St., Philadelphia, Pa. (33). F H
Wilson, Prof. Edmund B., Bryn Mawr, Pa. (36).
Wilson, James M., Riverton, Ill. (33).
Wilson, Dr. Jas. C., Cor. 17th and Samson Sts., Philadelphia, Pa. (33). F
Wilson, Mrs. M. V. C., Mobile, Ala. (37).
Winchell, Horace V., 10 State St., Minneapolis, Minn. (34). E C
Winchell, Martin R., 120 Nassau St., New York, N. Y. (36). I
Wingate, Miss Hannah S., 12 W. 125th St., New York, N. Y. (31). E I
Winslow, Arthur, Geol. Survey of Arkansas, Little Rock, Ark. (37). E
Winterburn, Geo. Wm., M.D., 29 W. 26th St., New York, N. Y. (36). F H
Winterhalter, A. G., Lt. U. S. N., U. S. Naval Observ., Washington, D. C. (37). A
Wisser, John P., 1st Lt. 1st Art'y, U. S. A., West Point, N. Y. (33). C
Withers, W. A., A.M., Agricultural Experiment Station, Raleigh, N. C. (33). C
Witthaus, Dr. R. A., 410 E. 26th St., New York, N. Y. (35).

Wolcott, Mrs. Henrietta L. T., Dedham, Mass. (29).
Wolf, Theo. R., Ph.D., Newark, Delaware (36).
Wolff, Dr. J. E., Cambridge, Mass. (36).
Wood, Harry A., M.D., State Insane Asylum, Buffalo, N. Y. (35).
Wood, Joseph S., Mount Vernon, Westchester Co., N. Y. (36).
WOOD, DR. ROBERT W., Jamaica Plain, Mass. (29).
WOOD, WALTER, 400 Chestnut St., Philadelphia, Pa. (33). F I
Wood, Dr. William B., 17 E. 38th St., New York, N. Y. (36).
Woodford, Prof. A. B., Indiana Univ., Bloomington, Ind. (36). I
Worthington, George, Editor Electrical Review, 13 Park Row, New York, N. Y. (36). B D
Wright, Rufus, 338-339 Lake St., Chicago, Ill. (37). B
Wright, Prof. Thos. W., Union College, Schenectady, N. Y. (36).
Würtele, Miss Minnie, Acton Vale, P. Q., Can. (32). H
Wyman, Walter Channing, 158 Dearborn St., Chicago, Ill. (34). H

Youmans, Mrs. Celia G., Mount Vernon, N. Y. (36).
Youmans, Wm. Jay, M.D., Popular Science Monthly, 1-5 Bond St., New York, N. Y. (28). F C
Young, Prof. A. Harvey, Hanover College, Hanover, Ind. (30). F C
Young, Albert B., 29 Park St., Buffalo, N. Y. (35).

Zalinski, E. L., 1st Lt., 5th Art'y, U. S. A., Fort Hamilton, N. Y. (36).
Zimmerman, William, 164 Dearborn St., Chicago, Ill. (30). F D B

[1274 PATRONS AND MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

HONORARY FELLOWS.[1]

ROGERS, WILLIAM B., Boston, Mass. (1). 1881. (Died in 1882.)
CHEVREUL, MICHEL EUGÈNE, Paris, France (35). 1886. (Died in 1889.)
GENTH, DR. F. A., 3937 Locust St., Philadelphia, Pa. (24). 1875. 1888. **C E**

FELLOWS.[2]

Aaron, Eugene M., Box 916, Philadelphia, Pa. (33). 1886. **F**
Abbe, Professor Cleveland, Army Signal Office, Washington, D. C. (16). 1874. **B A**
Abbott, Dr. Chas. C., Trenton, N. J. (29). 1883. **F H**
Adams, Frank Dawson, 141 Rideau St., Ottawa, Can. (29). 1885.
Alden, Prof. Geo. I., Worcester, Mass. (33). 1885. **D**
Alexander, John S., Texas Nat'l Bank, San Antonio, Texas (20). 1874. **B C D**
Allen, Dr. Harrison, 117 S. 20th St., Philadelphia, Pa. (29). 1882. **F**
Allen, Joel A., American Museum of Natural History, Central Park, New York (18). 1875. **F**
Allen, Dr. T. F., 10 E. 36th St., New York, N. Y. (35). 1887. **F**
Alvord, Major Henry E., Agricultural College, Md. (29). 1882. **I**
Ammen, Daniel, Rear Admiral U. S. Navy, Beltsville, Md. (26). 1881. **E**
Anderson, Dr. Joseph, Waterbury, Conn. (29). 1883. **H**
Anthony, Prof. Wm. A., Manchester, Conn. (28). 1880. **B**
Arthur, J. C., La Fayette, Ind. (21). 1883. **F**
Ashburner, Charles A., Penn Building, Pittsburgh, Pa. (31). 1883. **E**
Atkinson, Edward, 31 Milk St., Boston, Mass. (29). 1881. **I D**
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882. **C**
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29). 1886. **D A**
Ayres, Prof. Brown, Tulane University, New Orleans, La. (31). 1885. **B**

Babbitt, Miss Franc E., Lock Box 1284, Coldwater, Mich. (32). 1887. **H I**
Babcock, S. Moulton, Madison, Wis. (33). 1885. **C**
Bailey, Prof. Liberty H., Agricultural College, Mich. (34). 1887. **F**
Bailey, Prof. W. W., Brown University, Providence, R. I. (18). 1874. **F**
Baker, Frank, M.D., 1315 Corcoran St., Washington, D. C. (31). 1886. **F H**

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date following is the year when made a Fellow; the black letters at end of line are those of the sections to which the fellow belongs.

When the name is given in small capitals, it designates that the Fellow is also a Life Member, and is entitled to the Annual Volume of Proceedings.

Baker, Marcus, U. S. Geological Survey, Washington, D. C. (30). 1882. **A**

BARKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875. **B C**

Barnard, Edward E., Lick Observ., San José, Cal. (26). 1883. **A**

Barnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874. **B D I A F**

Barnes, Prof. Chas. R., Madison, Wis. (33). 1885 **F**

Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883. **C**

Bartlett, John R., Commander U. S. N., Navy Dep't, Washington, D. C. (30). 1882. **E B**

Barus, Carl, Ph.D., U. S. Geol. Survey, Washington, D. C. (33). 1887. **B**

Bassett, Homer F., Waterbury, Conn. (23). 1874. **F**

Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875. **B D I**

Bates, Henry Hobart, U. S. Patent Office, Washington, D. C. (33). 1887. **B A C D**

Bausch, Edward, Rochester, N. Y. (26). 1883. **A B C F**

Beal, Prof. Wm. James, Agricultural College, Ingham Co., Mich. (24). 1880. **F**

Beardsley, Prof. Arthur, Swarthmore College, Swarthmore, Del. Co., Pa. (33). 1885. **D**

Beauchamp, Rev. Wm. M., Baldwinsville, N. Y. (34). 1886. **H**

Bebb, M. S., 926 Grant Ave., Rockford, Ill. (34). 1886. **F**

Bell, Dr. Alex. Graham, Scott Circle, 1500 Rhode Island Ave., Washington, D. C. (26). 1879. **B H I**

Bell, Alex. Melville, 1525 35th St., Washington, D. C. (31). 1885. **H**

Bell, Samuel N., Manchester, N. H. (7). 1874.

Beman, Wooster W., 19 So. 5th St., Ann Arbor, Mich. (34). 1886. **A**

Benjamin, Marcus, 15 W. 121st St., New York, N. Y. (27). 1887. **C**

Benjamin, Rev. Raphael, M.A., 1334 Lexington Ave., New York, N. Y. (34). 1887. **F A B D E H I**

Bessey, Prof. Charles E., Univ. of Nebraska, Lincoln, Neb. (21). 1880. **F**

Bethune, Rev. C. J. S., Trinity College School, Pt. Hope, Ont., Can. (18). 1875. **F**

Beyer, Dr. Henry G., U. S. N., U. S. National Museum, Washington, D. C. (31). 1884. **F**

Bickmore, Prof. Albert S., American Museum of Natural History, 8th Ave. and 77th St., Central Park, New York, N. Y. (17). 1880. **H**

Bigelow, Prof. Frank H., Racine, Wis. (36) 1888. A

Billings, John S., Surgeon U. S. A., Surg. General's Office, Washington, D. C. (32). 1883. **F H**

Blackham, George E., M.D., Dunkirk, N. Y. (25). 1883. **F**

Blake, Clarence J., M.D., 226 Marlborough St., Boston, Mass. (24). 1877. **B F**

Blake, Prof. Eli W., Brown Univ., Providence, R. I. (15). 1874. **B**

Blake, Francis, Auburndale, Mass. (23). 1874. **B A**

Boardman, Mrs. William D., 38 Kenilworth St., Roxbury, Mass. (28). 1885. **E H**
Boas, Dr. Franz, 196 Third Ave., New York, N. Y. (36) 1888. **H**
Boerner, Chas. G., Vevay, Switzerland Co., Ind. (29). 1886. **A B E**
BOLTON, DR. H. CARRINGTON, University Club, New York, N. Y. (17). 1875. **C**
Bond, Geo. M., care of The Pratt & Whitney Co., Hartford, Conn. (33). 1885. **D**
Borden, Spencer, Fall River, Mass. (29). 1882. **B C I**
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bourke, John G., Capt. 3d Cavalry, U. S. A., War Dept., Washington, D. C. (33). 1885. **H**
Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875. **E**
Bowditch, Prof. H. P., Jamaica Plain, Mass. (28). 1880. **F B H**
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875. **F H**
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.
Brace, DeWitt B., Lincoln, Neb. (35). 1887. **B**
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875. **B**
Brackett, Solomon H., St. Johnsbury, Vt. (29). 1884. **B A**
Branner, John C., Director of the Geological Survey of Arkansas, Little Rock, Ark. (34). 1886. **E F**
Bransford, John Francis, Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (36) 1888. **H I**
Brashear, Jno. A., Allegheny, Pa. (33). 1885. **A B D**
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875. **E F I**
Brewster, William, 61 Sparks St., Cambridge, Mass. (29). 1884. **F**
Brinton, D. G., M.D., Media, Pa. (33). 1885. **H**
Britton, N. L., Columbia College, New York, N. Y. (29). 1882. **F E**
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879. **E**
Brooks, Wm. R., Geneva, N. Y. (35). 1886. **A**
Bross, Hon. Wm., Tribune Office, Chicago, Ill. (7). 1874. **E H**
Brown, Robert, care of Yale College Observatory, New Haven, Conn. (11). 1874.
Brown, Mrs. Robert, New Haven, Conn. (17). 1874.
Brühl, Gustav, cor. John and Hopkins Sts., Cincinnati, Ohio (28). 1886. **H**
Brush, Charles F., Brush Electric Light Co., Cleveland, Ohio (35). 1886. **B**
BRUSH, PROF. GEORGE J., Yale College, New Haven, Conn. (4). 1874. **C E**
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881. **F**
Burnham, S. W., Lick Observ., San José, Cal. (25). 1877. **A**
Burr, Prof. William H., Phoenixville, Chester Co., Pa. (31). 1883.
Burrill, Prof. T. J., Univ. of Illinois, Champaign, Ill. (29). 1882. **F**
Butler, A. W., Brookville, Franklin Co., Ind. (30). 1885. **F H**

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875. **C**
Campbell, Douglas H., 91 Alfred St., Detroit, Mich. (34) 1888. **F**
Campbell, Edw. D., Box 402, Sharon, Mercer Co., Pa. (34). 1887. **C**
Canby, William M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878. **F**

Carhart, Prof. Henry S., University of Michigan, Ann Arbor, Mich. (29). 1881. **B**
Carmichael, Prof. Henry, 7 Broad St., Boston, Mass. (21). 1875. **C**
Carpenter, Capt. W. L., U. S. A., Dunkirk, N. Y. (24). 1877. **F E**
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31). 1883. **B**
Carr, Lucien, Peabody Museum Archæology and Ethnology, Cambridge, Mass. (25). 1877. **H**
Case, Col. Theo. S., Kansas City, Mo. (27). 1883. **H B**
Chamberlin, T. C., Madison, Wis. (21). 1877. **E B F H**
Chandler, Prof. C. F., School of Mines, Columbia Coll., East 49th St. cor. 4th Ave., New York, N. Y. (19). 1875. **C**
Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883. **A B**
Chandler, Seth C., jr., 16 Cragie St., Cambridge, Mass. (29). 1882. **A**
Chandler, Prof. W. H., South Bethlehem, Pa. (19). 1874. **C**
Chanute, O., Kansas City, Mo. (17). 1877. **D I**
Chapin, Dr. J. H., Meriden, Conn. (33). 1886. **E H**
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29). 1882. **C F**
Chester, Prof. Fred'k D., Del. State Coll., Newark, Del. (33). 1887. **E**
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877. **F I**
Chittenden, Russell H., Ph.D., New Haven, Conn. (29). 1882. **C F**
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Mass. (31). 1883. **F**
Clark, Alvan G., Cambridgeport, Mass. (28). 1880. **A B**
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17). 1875. **A**
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874. **C**
Claypole, Prof. Edw. W., Buchtel Coll., Akron, Ohio (30). 1882. **E F**
Clayton, H. Helm, Readville, Mass. (34). 1887. **B**
Cloud, John W., Buffalo, N. Y. (28). 1886. **A B D**
Coffin, Prof. John H. C., U. S. Navy, 1901 I St., Washington, D. C. (1). 1874. **A**
Coffin, Prof. Selden J., Lafayette College, Easton, Pa. (22). 1874. **A I**
Collett, Prof. John, Indianapolis, Ind. (17). 1874. **E**
Collingwood, Francis, Elizabeth, N. J. (36). 1888. **D**
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880. **E**
Comstock, Prof. Geo. C., Washburn Observ., Univ. of Wisconsin, Madison, Wis. (34). 1887. **A**
Comstock, J. Henry, Cornell Univ., Ithaca, N. Y. (28). 1882. **F**
Comstock, Prof. Theo. B., Prof. of Mining Engineering, Univ. of Illinois, Champaign, Ill. (24). 1877. **D E B**
Cook, Prof. A. J., Agricultural College, Mich. (24). 1880. **F**
Cook, Prof. George H., New Brunswick, N. J. (4). 1875. **E**
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880. **B C**

Cooley, Prof. Mortimer E., Univ. of Michigan, Ann Arbor, Mich. (33). 1885. **D**

Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1875. **F E**

Corthell, Elmer L., 205 La Salle St., Chicago, Ill. (34). 1886. **D**

Coulter, Prof. John M., Wabash Coll., Crawfordsville, Ind. (32). 1884. **F**

Cox, Prof. Edward T., New Harmony, Ind. (19). 1874. **E**

Cox, Hon. Jacob D., Gilman Ave., Mt. Auburn, Cincinnati, Ohio (30). 1881. **F**

Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879. **D E**

Crampton, Chas. A., Dept. of Agric., Washington, D. C. (36). 1887. **C**

Crandall, Prof. A. R., Lexington, Ky. (29). 1883. **E F**

Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874. **E F**

Crosby, Prof. Wm. O., Boston Society of Natural History, Boston, Mass. (29). 1881. **E**

Cross, Prof. Chas. R., Mass. Institute Technology, Boston, Mass. (29). 1880. **B**

Cummings, Rev. Joseph, D.D., President Northwestern University, Evanston, Ill. (13). 1874. **I H**

Cushing, Henry Platt, Box 482, Mankato, Minn. (33). 1888. **E**

Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17). 1874. **E F**

Dabney, Chas. W., jr., Ph.D., Agricultural Experiment Station, Raleigh, N. C. (30). 1882. **C B E**

Dall, Mrs. Caroline H., 1630 O St., Washington, D. C. (18). 1874. **F H**

Dall, William H., Smithsonian Institution, Washington, D. C. (18). 1874. **H F**

Dana, Edward Salisbury, New Haven, Conn. (23). 1875. **B E**

Dana, Prof. James D., New Haven, Conn. (1). 1875. **E**

Danforth, Edward, Department of Public Instruction, Elmira, N. Y. (11). 1874. **E I H A B**

Davenport, B. F., M.D., 161 Tremont St., Boston, Mass. (29). 1883. **C**

Davidson, Prof. Geo., U. S. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881. **A B D**

Davis, Wm. Morris, Cambridge, Mass. (33). 1885. **E B**

Dawson, Sir William, Principal McGill College, Montreal, Can. (10). 1875. **E**

Day, David F., Buffalo, N. Y. (35). 1887. **F**

Day, F. H., M.D., Wauwatosa, Wis. (20). 1874. **E H F**

Dean, George W., P. O. Box 92, Fall River, Mass. (15). 1874. **A**

Denton, Prof. James E., Stevens Institute, Hoboken, N. J. (36) 1888. **D B A**

Dewey, Fred P., Ph.B., Smithsonian Institution, Washington, D. C. (30). 1886. **C E**

Diller, J. Silas, U. S. Geol. Survey, Washington, D. C. (29). 1884. **E**

Dimmock, George, Cambridge, Mass. (22). 1874. **F**

Dinwiddie, Robert, 117 W. 43d St., New York, N. Y. (1). 1874. **F**

Dodge, Charles R., Washington, D. C. (22). 1874.

Dodge, Prof. James A., University of Minnesota, Minneapolis, Minn. (29). 1884. **C E**

Dodge, J. Richards, Washington, D. C. (31). 1884. **I H**

Dolbear, A. Emerson, College Hill, Mass. (20). 1880. **B**

Doolittle, Prof. C. L., South Bethlehem, Pa. (25). 1885. **A**

Dorsey, Rev. J. Owen, Box 591, Washington, D. C. (31). 1883. **H**

Douglass, Andrew E., P. O. Box 605, New York, N. Y. (31). 1885. **H**

Dow, Capt. John M., 69 Seventh Ave., New York, N. Y. (31). 1884. **F H**

DRAPER, DAN'L, Ph. D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Avenue, New York, N. Y. (29). 1881. **B D F A**

Drown, Prof. Thos. M., Mass. Institute Technology, Boston, Mass. (29). 1881. **C**

DU BOIS, PROF. AUG. J., New Haven, Conn. (30). 1882. **A B D**

Du Bois, Patterson, Ass't Editor S.S.T., 1031 Walnut St., Philadelphia, Pa. (33). 1887. **H C I**

Dudley, Charles B., Altoona, Pa. (23). 1882. **C B D**

Dudley, P. H., 66½ Pine St., New York, N. Y. (29). 1884.

DUDLEY, WM. L., Prof. of Chemistry, Vanderbilt Univ., Nashville, Tenn. (28). 1881. **C**

Dudley, Prof. Wm. R., Ithaca, N. Y. (29). 1883. **F**

Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880. **C**

Dutton, Capt. C. E., U. S. Geol. Surv., Washington, D. C. (23). 1875.

Dwight, Prof. William B., Vassar College, Poughkeepsie, N. Y. (30). 1882. **E F**

Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879. **A**

Eaton, Prof. D. G., 55 Pineapple St., Brooklyn, N. Y. (19). 1874. **B E**

Eaton, Prof. James R., Liberty, Mo. (29). 1885. **C B E**

Eddy, Prof. H. T., Univ. of Cincinnati, Cincinnati, O. (24). 1875. **A B D**

Edison, Thos. A., Menlo Park, N. J. (27). 1878. **B**

Edmands, J. Rayner, Observatory, Cambridge, Mass. (29). 1880.

Egleston, Prof. Thomas, 35 W. Washington Square, New York, N. Y. (27). 1879. **C D E**

Eimbeck, William, U. S. C. and G. S., Washington, D. C. (17). 1874. **A B D**

Elkin, William L., Yale Coll. Observ., New Haven, Conn. (33). 1885. **A**

Elliott, Arthur H., 591 Broadway, care Anthony's Bulletin, New York, N. Y. (23). 1880. **C**

Ely, Theo. N., Sup't Motive Power, Penn. R. R., Altoona, Pa. (29). 1886.

Emerson, Prof. Benjamin K., Amherst, Mass. (19). 1877. **E F**

Emerson, Prof. C. F., Dartmouth Coll., Hanover, N. H. (22). 1874. **B A**

Emerton, James H., 11 St. James Ave., Boston, Mass. (18). 1875. **F**

Emery, Albert H., Stamford, Conn. (29). 1884. **D B**

Emery, Charles E., 22 Cortlandt St., New York, N. Y. (34). 1886. **D B A**

EMMONS, S. F., U. S. Geol. Survey, Washington, D. C. (26). 1879. **E**

Engelmann, George J., M.D., 3003 Locust St., St. Louis, Mo. (25). 1875. **F H**

Evans, Asher B., 500 Pine St., Lockport, N. Y. (19). 1874. **A**

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874. **B D A**

Fairchild, Prof. H. L., University of Rochester, Rochester, N. Y. (28). 1883. **E F**

Fall, Prof. Delos, Albion College, Albion, Mich. (34). 1887. **C**

Fanning, John T., Union Depot Building, Minneapolis, Minn. (29). 1885. **D**

Farlow, Dr. W. G., 29 Holyoke House, Cambridge, Mass. (20). 1875. **F**

Farmer, Moses G., Eliot, Me. (9). 1875.

Farquhar, Henry, Coast Survey Office, Washington, D. C. (33). 1886. **A I F B**

Fernald, Prof. Charles H., Amherst, Mass. (22). 1881. **F E**

Fernald, Prof. M. C., State Agric. College, Orono, Me. (22). 1883. **B A**

Fernow, Bernhard E., Chief of Forestry Division, Dep't of Agriculture, Washington, D. C. (31). 1887. **F I**

Ferrel, Wm., 1641 Broadway, Kansas City, Mo. (11). 1875. **A B**

Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878. **A B**

Fine, Prof. Henry B., College of New Jersey, Princeton, N. J. (35). 1887. **A**

Firmstone, F., Easton, Pa. (33). 1887. **D**

Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874. **I E**

Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883. **H**

Fletcher, James, Dep't of Agriculture, Ottawa, Can. (31) 1883. **F**

Fletcher, Dr. Robert, Surgeon General's Office, U. S. A., Washington, D. C. (29). 1881. **F H**

Flint, Albert S., U. S. Naval Observ., Washington, D. C. (30). 1887. **A**

Flint, James M., Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (28). 1882. **F**

Foote, Dr. A. E., 1223 Belmont Ave., Philadelphia, Pa. (21). 1874. **E C**

Forbes, Prof. S. A., Univ. of Illinois, Champaign, Ill. (27). 1879. **F**

Fox, Prof. Joseph G., Lafayette College, Easton, Pa. (31). 1886. **A B D**

Foye, Prof. J. C., Lawrence Univ., Appleton, Wis. (29). 1884. **C B**

FRAZER, DR. PERSIFOR, 917 Clinton St., Philadelphia, Pa. (24). 1879. **E C**

Frazier, Prof. B. W., The Lehigh Univ., Bethlehem, Pa. (24). 1882. **E C**

Frear, Wm., State College, Centre Co., Pa. (33). 1886. **C**

French, Prof. Thomas, jr., Ridgeway Ave., Avondale, Cincinnati, Ohio (30). 1883. **B**

Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880. **A**

Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882. **F**

Fulton, Prof. Robert B., University, Miss. (21). 1887. **B A**

Gage, Simon Henry, Ithaca, N. Y. (28). 1881. **F**

Gannett, Henry, U. S. Geological Survey, Washington, D. C. (33). 1884. **E I A**

Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874. **C B**
Gardiner, James T., 21 Elk St., Albany, N. Y. (25). 1879. **E**
Garland, Rev. Dr. L. C., Chancellor Vanderbilt University, Nashville, Tenn. (25). 1877 **B**
Garman, Samuel, Museum Comparative Zoology, Cambridge, Mass. (20). 1874. **F E**
Gatschet, Dr. Albert S., Box 333, Washington, D. C. (30). 1882. **H**
Gibbs, Prof. J. Willard, New Haven, Conn. (33). 1885. **B**
Gilbert, G. K., Box 591, Washington, D. C. (18). 1874. **E**
Gill, Prof. Theo., Smithsonian Institution, Washington, D. C. (17). 1874.
Gillman, Henry, U. S. Consul, Jerusalem, Palestine, via England and Brindisi. (24). 1875. **H F**
Gilman, Daniel C., President Johns Hopkins University, Baltimore, Md. (10). 1875. **E H**
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875. **C**
Gold, Theodore S., West Cornwall, Conn. (4). 1887. **B C**
Goldschmidt, S. A., Ph.D., 55 Broadway, New York, N. Y. (24). 1880. **C E B**
Gooch, Frank A., Yale College, New Haven, Conn. (25). 1880. **C**
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, G. Brown, Curator Nat'l Museum, Washington, D. C. (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879. **A H**
Gould, Dr. B. A., Cambridge, Mass. (2). 1875. **A B**
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874. **A**
Gratacap, L. P., Ph.B., 77th St. and 8th Ave., New York, N. Y. (27). 1884. **C E F**
Gray, Elisha, Sc.D., Highland Park, Ill. (32). 1883. **B**
Green, Arthur L., La Fayette, Ind. (33). 1888. **C**
Green, Traill, M.D., Easton, Pa. (1). 1874. **C F**
Grimes, J. Stanley, Evanston, Cook Co., Ill. (17). 1874. **E H**
Grinnell, George Bird, 40 Park Row, New York, N. Y. (25). 1885. **F E**
Gulley, Prof. Frank A., College Station, Texas. (30). 1883.

Hagen, Dr. Hermann A., Museum Comparative Zoology, Cambridge, Mass. (17). 1875. **F**
Hague, Arnold, U. S. Geol. Survey, Washington, D. C. (26). 1879.
Hale, Albert C., Ph.D., No. 356 Carlton Ave., Brooklyn, N. Y. (29). 1886. **C B**
Hale, Horatio, Clinton, Ontario, Can. (30). 1882. **H**
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877. **A**
Hall, Prof. C. W., 803 Univ. Ave. So., Minneapolis, Minn. (28). 1883. **E**
Hall, Prof. Edwin H., 5 Avon St., Cambridge, Mass. (29). 1881. **B**
Hall, Prof. James, Albany, N. Y. (1). 1875. **E F**
Hall, Prof. Lyman B., Haverford College, Pa. (31). 1884. **C**
Halsted, Byron D., New Jersey Agricultural Experiment Station, New Brunswick, N. J. (29). 1888. **F**

Hamlin, Dr. A. C., Bangor, Me. (10). 1874. **C E H**
HANAMAN, C. E., Troy, N. Y. (19). 1883. **F**
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883. **A**
Harger, Oscar, 14 University Place, New Haven, Conn. (25). 1879. **F E**
HARKNESS, PROF. WILLIAM, U. S. N. Observatory, Washington, D. C. (26). 1878. **A B C D**
Harris, Uriah R., Lieutenant U. S. N., Navy Yard, Mare Island, Vallejo, Cal. (34). 1886. **A**
Harris, W. T., Lock Box 1, Concord, Mass. (27). 1887. **H I**
Hart, Edw., Ph.D., Easton, Pa. (33). 1885. **C**
Hasbrouck, Prof. I. E., 364 Carlton Ave., Brooklyn, N. Y. (28). 1874. **D A I**
HASTINGS, C. S., Sheffield Scientific School of Yale College, New Haven, Conn. (25). 1878. **B**
Haupt, Prof. Lewis M., University of Pennsylvania, Philadelphia, Pa. (32). 1885. **I D E**
Haynes, Henry W., 239 Beacon St., Boston, Mass. (28). 1884. **H**
Heilprin, Prof. Angelo, Acad. Nat. Sciences, Philadelphia, Pa. (33). 1885. **E F**
Hendricks, J. E., Des Moines, Iowa (29). 1885. **A**
Henshaw, Henry W., Bureau of Ethnology, Washington, D. C. (24). 1877. **H**
Hering, Rudolph, 31 Chambers St., Room 19, New York, N. Y. (33). 1885. **D E I**
Herrick, Clarence L., Granville, Ohio (31). 1884. **F E**
Hervey, Rev. A. B., Taunton, Mass. (22). 1879. **F**
Hicks, Prof. Lewis E., State University, Lincoln, Neb. (31). 1885. **E F**
Hilgard, Prof. E. W., University of California, Berkeley, Cal. (11). 1874. **C E B**
Hilgard, Prof. J. E., Washington, D. C. (4). 1874. **A**
Hill, Chas. S., care Dep't of State, Washington, D. C. (33). 1887. **A I**
Hill, Rev. Dr. Thomas, 738 Congress St., Portland, Me. (3). 1875. **A**
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882. **B C**
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874. **E**
Hitchcock, Romyn, Washington, D. C. (29). 1881. **C B**
Hobbs, A. C., Bridgeport, Conn. (28). 1886. **D**
Hodges, N. D. C., Editorial office of Science, 47 Lafayette St., New York, N. Y. (29). 1882. **B**
Hoffmann, Dr. Fred., 183 Broadway, New York, N. Y. (28). 1881. **C F**
Holden, Prof. E. S., Lick Observatory, San José, Cal. (23). 1875. **A**
Holman, Silas W., Massachusetts Institute of Technology, Boston, Mass. (31). 1883. **B**
Holmes, Prof. Jos. A., Chapel Hill, N. C. (33). 1887. **E F**
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881. **H**
Holmes, Wm. H., Bureau of Ethnology, Smithsonian Institution, Washington, D. C. (30). 1883. **H**
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876. **C E**

Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30). 1883.
Hotchkiss, Major Jed., Staunton, Va. (31). 1883. **E H I**
Hough, Prof. G. W., Director Dearborn Observatory, Chicago, Ill. (15). 1874. **A**
Hovey, Rev. Horace C., 14 Park St., Bridgeport, Conn. (29). 1883. **E H**
Howe, Jas. Lewis, Louisville, Ky. (36). 1888. **C**
Hoy, Philo R., M.D., 902 Main St., Racine, Wis. (17). 1875. **F H**
Hulst, Rev. Geo. D., 15 Himrod St., Brooklyn, N. Y. (29). 1887. **F**
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Park Avenue Hotel, New York, N. Y. (1). 1874. **C E**
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875. **E**
Hyde, Prof. E. W., Station D, Cincinnati, Ohio (25). 1881. **A**

Iddings, Joseph P., U. S. Geol. Survey, Washington, D. C. (31). 1884. **E**
Irving, Roland D., Wisconsin State Geol. Survey, Madison, Wis. (26). 1879. **E**

James, Edmund J., Ph.D., Univ. of Pa., Philadelphia, Pa. (33). 1887. **I**
James, Jos. F., M.S., Agricultural College, Prince George's Co., Md. (30). 1882. **F E**
Jastrow, Dr. Jos., Johns Hopkins Univ., Baltimore, Md. (35). 1887. **H F**
Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29). 1884. **F H**
Jeffries, B. Joy, M.D., 15 Chestnut St., Boston, Mass. (29). 1881. **F H**
Jenkins, Edw. H., New Haven, Conn. (33). 1885. **C**
Jenks, Elisha T., Middleborough, Mass. (22). 1874. **D**
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874. **B**
Jewell, Theo. F., Commander U. S. N., Torpedo Station, Newport, R. I. (25). 1882. **B**
Jillson, Dr. B. C., 342 River Ave., E.E., Pittsburgh, Pa. (14). 1881. **E H F**
Johnson, John B., Washington Univ., St. Louis, Mo. (33). 1886. **D**
Johnson, Lawrence C., 1330 F St., N. W., Washington, D. C. (33). 1887.
Johnson, Otis C., Ann Arbor, Mich. (34). 1886. **C**
Johnson, Prof. S. W., 54 Trumbull St., New Haven, Conn. (22). 1874. **C**
Johnson, Prof. W. W., Naval Academy, Annapolis, Md. (29). 1881. **A**
Joy, Prof. Charles A., care F. Hoffmann, Stockbridge, Mass. (8). 1879.
Julien, A. A., New York Acad. of Sciences, New York, N. Y. (24). 1875. **E C**

Kedzie, Prof. Robert C., Agricultural College, Mich. (29). 1881. **C**
Keiser, Edward H., Ph.D., Prof. of Chemistry, Bryn Mawr College, Bryn Mawr, Montgomery Co., Pa. (35). 1888. **C**
Kellicott, David S., Columbus, Ohio. (31). 1883. **F**
Kemp, James F., Cornell Univ., Ithaca, N. Y. (36). 1888. **E**
Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882. **A**
Kent, William, Passaic, N. J. (26). 1881. **D I**
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883. **A B**

Kimball, Arthur Lalanne, Johns Hopkins Univ., Baltimore, Md. (33). 1885. **B**

Kingsley, Prof. J. Sterling, Bloomington, Ind. (33). 1886. **F**

Kinnicutt, Dr. Leonard P., Polytechnic Inst., Worcester, Mass. (28). 1883. **C**

Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874. **A**

Kunz, G. F., with Tiffany & Co., New York, N. Y. (29). 1883. **E H C**

Laflamme, Prof. J. C. K., Laval Univ., Quebec, Can. (29). 1887. **E B**

LaFlesche, Francis, Indian Bureau, Interior Dep't, Washington, D. C. (33). 1885. **H**

Lambert, Rev. Thomas R., D.D., Hotel Oxford, Huntington Ave., Boston, Mass. (18). 1874.

Landreth, Prof. Olin H., Vanderbilt Univ., Nashville, Tenn. (28). 1883. **D**

Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882. **F H**

Langley, Prof. J. W., Univ. of Mich., Ann Arbor, Mich. (23). 1875. **C B**

Langley, Prof. S. P., Secretary Smithsonian Institution, Washington, D. C. (18). 1874. **A B**

Lanza, Prof. Gaetano, Mass. Institute of Technology, Boston, Mass. (29). 1882. **D A B**

Larkin, Edgar L., Director Knox College Observatory, Galesburg, Ill. (28). 1883. **A**

Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15). 1874. **C**

Lawrence, George N., 45 E. 21st St., New York, N. Y. (7). 1877. **F**

Lazenby, Prof. Wm. R., Columbus, Ohio (30). 1882. **B I**

Leavenworth, Francis P., Haverford College P. O., Montgomery Co., Pa. (30). 1888. **A**

LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881. **E F**

Ledoux, Albert R., Ph.D., 10 Cedar St., New York, N. Y. (26). 1881. **C**

Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874. **C F**

Lehman, G. W., Ph.D., 111 S. Gay St., Baltimore, Md. (30). 1885. **C B**

Lesley, Prof. J. Peter, State Geologist of Pennsylvania, 1008 Clinton St., Philadelphia, Pa. (2). 1874. **E**

Libbey, Prof. William, jr., Princeton, N. J. (29). 1887. **E F**

LILLY, GEN. WM., Mauch Chunk, Carbon Co., Pa. (28). 1882. (Patron). **F E**

Lindsley, J. Berrien, M.D., 135 North Spruce St., Nashville, Tenn. (1). 1874. **F**

Lintner, J. A., N. Y. State Entomologist, Room 27, Capitol, Albany, N. Y. (22). 1874. **F**

Litton, Abram, 2220 Eugenia St., St. Louis, Mo. (28). 1879. **C**

Lockwood, Samuel, Ph.D., Freehold, Monmouth Co., N. J. (18). 1875. **F B A**

Loomis, Prof. Elias, New Haven, Conn. (1). 1874. **A B**

Loughridge, Dr. R. H., South Carolina College, Columbia, S. C. (21). 1874. **E C**

Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882. C

Lovering, Prof. Joseph, Harv. Univ., Cambridge, Mass. (2). 1875. B A

Lupton, Prof. N. T., Auburn, Ala. (17). 1874. C

Lyle, David Alexander, Captain, Ordnance Dept. U. S. A., Ordnance Office, Washington, D. C. (28). 1880. D

Lyman, Prof. Chester S., 88 Trumbull St., New Haven, Conn. (4). 1875. A

Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 321 Michigan Ave., Chicago, Ill. (29). 1881.

McGee, W. J., U. S. Geol. Survey, Washington, D. C. (27). 1882. E

McGill, John T., Ph.D., Vanderbilt Univ., Nashville, Tenn. (36). 1888. C

McLeod, C. H., McGill Univ., Montreal, Can. (35). 1887.

McMurtrie, William, Univ. of Illinois, Champaign, Ill. (22). 1874. C

McNeill, Malcolm, Princeton, N. J. (32). 1885. A

Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881. C

Macfarlane, Prof. A., Univ. of Texas, Austin, Texas (34). 1886. B A

Mackintosh, James B., Consolidated Gas Co., 21st St. and Ave. A, New York, N. Y. (27). 1883. C B

Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882. F

Magie, Prof. William F., College of New Jersey, Princeton, N. J. (35). 1887.

Mallery, Col. Garrick, U. S. Army, Bureau of Ethnology, Washington, D. C. (26). 1879. H

MANN, B. PICKMAN, 1918 Sunderland Place, Washington, D. C. (22). 1874. I F

Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874. E

Markoe, Prof. Geo. F. H., 29 Montrose St., Roxbury, Mass. (29). 1881.

MARSH, PROF. O. C., Yale College, New Haven, Conn. (15). 1874. F H

Martin, Prof. Daniel S., 236 West 4th St., New York, N. Y. (28). 1879. E F

Martin, Prof. H. Newell, Johns Hopkins University, Baltimore, Md. (27). 1880. F H

Martin, Miss Lillie J., High School, Indianapolis, Ind. (32). 1886. F C

Martin, Prof. Wm. J., Davidson College, N. C. (31). 1884. C E

Mason, Prof. Otis T., National Mus., Washington, D. C. (25). 1877. H

Mason, Dr. William P., Prof. Rensselaer Polytechnic Inst., Troy, N. Y. (31). 1886. C

Matthews, Washington, 1262 New Hampshire Ave., cor. 21st St., N. W., Washington, D. C. (37). 1888. H

Maxwell, Rev. Geo. M., Wyoming, Hamilton Co., Ohio (30). 1886. H E

Mayer, Prof. A. M., South Orange, N. J. (19). 1874.

Meehan, Thomas, Germantown, Pa. (17). 1875. F

Mees, Prof. Carl Leo, Columbus, Ohio (24). 1876. B C

Mendenhall, Prof. T. C., Rose Polytechnic Inst., Terre Haute, Ind. (20). 1874. B
Menocal, Aniceto G., C. E., U. S. N., Navy Yard, Washington, D. C. (36). 1888. D
Merriam, C. Hart, M.D., Smith. Inst., Washington, D. C. (33). 1885. F
Merrill, Frederick J. H., Ph.B., 126 E. 60th St., New York, N. Y. (35). 1887. E
Merriman, C. C., 1910 Surf St., Lake View, Chicago, Ill. (29). 1880. F
Merriman, Prof. Mansfield, Lehigh University, Bethlehem, Pa. (32). 1885. A D
Metz, Charles L., M.D., Madisonville, Hamilton Co., Ohio (30). 1885. H
Michael, Mrs. Arthur, 1509 Locust St., Philadelphia, Pa. (33). 1885. C F
Michelson, A. A., Master U. S. N., 7 Rockwell St., Cleveland, Ohio (26). 1879. B
Mills, T. Wesley, Montreal, Can. (31). 1886. F H
Minot, Dr. Charles Sedgwick, 25 Mt. Vernon St., Boston, Mass. (28). 1880. F
Minot, Francis, M.D., 65 Marlborough St., Boston, Mass. (29). 1884.
Mitchell, Miss Maria, 87 Green St., Lynn, Mass. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882. C
Moore, Prof. J. W., M.D., Lafayette College, Easton, Pa. (22). 1874. B D A
Moore, Robert, C.E., 325 Chestnut St., St. Louis, Mo. (33). 1887. D B I
Morley, Prof. Edward W., 749 Republic St., Cleveland, Ohio (18). 1876. C B E
Morong, Rev. Thomas, Ashland, Mass. (35). 1887. F
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874. F H
Morton, H., Stevens Inst. Technology, Hoboken, N. J. (18). 1875. B C
Moses, Prof. Thos. F., Urbana Univ., Urbana, Ohio (25). 1883. H F
Munroe, Prof. C. E., Chemist to Bureau of Ordnance, U. S. Torpedo Station, Newport, R. I. (22). 1874. C
Murdoch, John, Smithsonian Institution, Washington, D. C. (29). 1886. F H
Murdock, J. B., Lieut. U. S. N., 24 Alaska St., Roxbury, Mass. (28). 1885. B
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881. F

Nason, Frank L., 5 Union St., New Brunswick, N. J. (36) 1888. E
Nason, Prof. H. B., Rensselaer Polytechnic Institute, Troy, N. Y. (18). 1874. C E
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882. A B D
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877. E F
Newberry, Prof. J. S., Columbia College, New York, N. Y. (5). 1875. E F H I
Newberry, Prof. Spencer Baird, Ithaca, N. Y. (33). 1887. C
Newcomb, Prof. S., Navy Dep't, Washington, D. C. (18). 1874. A B

Newton, Hubert A., New Haven, Conn. (6). 1874. **A**

Nichols, E. L., Ph.D., Cornell Univ., Ithaca, N. Y. (28). 1881. **B C**

Nicholson, Prof. H. H., Box 675, Lincoln, Neb. (36). 1888.

Niles, Prof. W: H., Cambridge, Mass. (16). 1874.

Nipher, Prof. F. E., Washington Univ., St. Louis, Mo. (24). 1876. **B**

Norton, Lewis M., Ph.D., Mass. Institute of Technology, Boston, Mass. (29). 1884. **C**

NORTON, PROF. THOMAS H., Univ. of Cincinnati, Cincinnati, Ohio (35). 1887. **C**

Noyes, Prof. Wm. A., Rose Polytechnic Inst., Terre Haute, Ind. (32). 1885. **C**

Nuttall, Mrs. Zelia, care Peabody Museum, Cambridge, Mass. (35). 1887. **H**

Oliver, Charles A., M.D., 1507 Locust St., Philadelphia, Pa. (33). 1886. **F H B**

Oliver, Prof. James E., P. O. Box 1566, Ithaca, N. Y. (7). 1875. **A B I**

Ordway, Prof. John M., Tulane Univ., New Orleans, La. (9). 1875. **C**

Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875. **E**

Osborn, Henry F., S.D., Princeton, N. J. (29). 1883.

Osborn, Henry Leslie, 3 East 47th St., Hamline, Minn. (29). 1887.

Osborn, Herbert, Ames, Iowa (32). 1884. **F**

Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874. **D C B**

Owen, Dr. Richard, New Harmony, Ind. (20). 1874. **E**

Packard, Dr. A. S., 115 Angell St., Providence, R. I. (16). 1875. **F E**

Paine, Cyrus F., 305 Ellwanger & Barry Building, Rochester, N. Y. (12). 1874. **B A**

Paine, Nathaniel, Worcester, Mass. (18). 1874. **H**

Palfray, Hon. Charles W., Salem, Mass. (21). 1874.

Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen. U. S. A., Office of Chief of Engineers, Washington, D. C. (29). 1881. **D**

PARKHURST, HENRY M., Law Stenographer, 25 Chambers St., New York, N. Y. (23). 1874. **A**

Paul, Prof. Henry M., U. S. Naval Observatory, Washington, D. C. (33). 1885. **A B**

Peabody, Cecil H., Ass't Prof. Steam Eng., Mass. Institute Technology, Boston, Mass. (32). 1887. **D**

Peabody, Selim H., Regent University of Illinois, Champaign, Ill. (15). 1885. **D B F**

Peckham, S. F., 159 Olney St., Providence, R. I. (18). 1875. **C B E**

Pedrick, Wm. R., Lawrence, Mass. (22). 1875.

Peet, Rev. Stephen D., Mendon, Ill. (24). 1881. **H**

Peirce, Benj. O., jr., Ass't Prof., Harvard College, Cambridge, Mass. (29). 1886. **A B**

Pengra, Charles P., M.D., 130 Dartmouth St., Boston, Mass. (34). 1887. **F C**

Perkins, Prof. George H., Burlington, Vt. (17). 1882. **H F E**

Peter, Dr. Robert, Kentucky Geol. Survey, Lexington, Ky. (29). 1881. **C**

Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875. **E**

Philbrick, Edw. S., Brookline, Mass. (29). 1886. **D**

Phillips, A. W., New Haven, Conn. (24). 1879.

Phillips, Henry, jr., 320 So. 11th St., Philadelphia, Pa. (32). 1887. **H I**

Phippen, Geo. D., Salem, Mass. (18). 1874. **F**

Pickering, Prof. E. C., Director of Observatory, Cambridge, Mass. (18). 1875. **A B**

Pickering, William H., Harvard Observatory, Cambridge, Mass. (29). 1883. **B A**

Pilling, James C., Box 591, Washington, D. C. (28). 1882. **F H I**

Pillsbury, Prof. John H., Smith College, Northampton, Mass. (23). 1885. **F H**

Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882. **E**

Pohlman, Dr. Julius, Buffalo, N. Y. (32). 1884. **E F**

Porter, Thos. C., LL.D., Lafayette College, Easton, Pa. (33). 1887. **F**

Powell, Major J. W., U. S. Geologist, 910 M St. N. W., Washington, D. C. (23). 1875. **E H**

Power, Prof. Frederick B., Univ. of Wis., Madison, Wis. (31). 1887. **C**

Prentiss, Prof. A. N., Cornell Univ., Ithaca, N. Y. (35). 1887. **F**

Prentiss, D. Webster, M.D., 1101 14th St. N. W., Washington, D. C. (29). 1882. **F**

Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875. **C**

Pritchett, Henry S., Director Observatory Washington University, St. Louis, Mo. (29). 1881. **A**

Procter, John R., Dir. Kentucky Geol. Surv., Frankfort, Ky. (26). 1881.

Pulsifer, Wm. H., St. Louis, Mo. (26). 1879. **A H**

Pumpelly, Prof. Raphael, U. S. Geological Survey, Newport, R. I. (17). 1875. **E I**

Putnam, Prof. F. W., Curator Peabody Museum American Archæology and Ethnology, Cambridge, Mass. (Address as Permanent Secretary A. A. A. S., Salem, Mass.) (10). 1874. **H**

Pynchon, Rev. T. R., Pres. Trinity Coll., Hartford, Conn. (23). 1875.

Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Springfield, Ill. (11). 1875.

Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875. **E I**

Redfield, J. H., 216 W. Logan Square, Philadelphia, Pa. (1). 1874. **F**

Reed, E. Baynes, London, Ontario, Can. (27). 1882.

Rees, Prof. John K., Columbia College, New York, N. Y. (26). 1878. **A E B**

Remsen, Prof. Ira, Johns Hopkins Univ., Baltimore, Md. (22). 1875. **C**

Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881. **A D**

Rice, Prof. Wm. North, Wesleyan University, Middletown, Conn. (18). 1874. **E F**

Richards, Prof. Charles B., 313 York St., New Haven, Conn. (33). 1885. **D**

Richards, Edgar, Office of Internal Revenue, Treasury Dept., Washington, D. C. (31). 1886. **C**

Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875. **D**

Richards, Mrs. Robert H., Prof. Mass. Inst. of Tech., Boston, Mass. (23). 1878. **C**

Richardson, Clifford, Dep't of Agric., Washington, D. C. (30). 1884. **C**

Ricketts, Prof. Palmer C., Rensselaer Polytechnic Inst., Troy, N. Y. (33). 1887. **D A**

Ricketts, Prof. Pierre de Peyster, School of Mines, Columbia College, New York, N. Y. (26). 1880. **C D E**

RILEY, PROF. C. V., U. S. Entomologist, 1700 13th St. N. W., Washington, D. C. (17). 1874. **F H I**

Ritchie, E. S., Newton Highlands, Mass. (10). 1877. **B**

Roberts, Prof. Isaac P., Ithaca, N. Y. (33). 1886. **I**

Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30). 1883. **D B A**

Rockwell, Gen. Alfred P., Manchester, Mass. (10). 1882. **E**

Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883. **A D**

Rockwood, Prof. Charles G., jr., College of New Jersey, Princeton, N. J. (20). 1874. **A E B D**

Rogers, Fairman, Messrs. Dick Bros. & Co., No. 147 S. Fourth St., Philadelphia, Pa. (11). 1874.

Rogers, Prof. W. A., Colby Univ., Waterville, Me. (15). 1875. **A B D**

Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879. **E**

Rood, Prof. O. N., Columbia College, New York, N. Y. (14). 1875. **B**

Ross, Waldo O., 31 Otis St., Boston, Mass. (29). 1882.

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.

Runkle, Prof. J. D., Mass. Institute of Technology, Boston, Mass. (2). 1875. **A D**

Russell, I. C., U. S. Geological Survey, Washington, D. C. (25). 1882.

Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Sadtler, Prof. Sam'l P., Univ. of Pa., Philadelphia, Pa. (22). 1875. **C**

Safford, Dr. James M., Nashville, Tenn. (6). 1875. **E C F**

Salmon, Daniel E., Dep't of Agric., Washington, D. C. (31). 1885. **F**

Sampson, Commander W. T., U. S. N., Naval Acad., Annapolis, Md. (25). 1881. **B A**

Sanborn, Jeremiah Wilson, Agric. Coll., Columbia, Mo. (31). 1886.

Sanborn, Rev. John W., Lockport, N. Y. (33). 1886. **H**

Saunders, William, Director Agricultural Experiment Station, Ottawa, Canada (17). 1874. **F**

Schaeberle, J. M., Astronomer in the Lick Observatory, San José, Cal. (34). 1886. **A**

Schanck, Prof. J. Stillwell, Princeton, New Jersey (4). 1882. C B H

Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874. A

Schweitzer, Prof. Paul, State University of Missouri, Columbia, Mo. (24). 1877. C B

Scott, Prof. Wm. B., Princeton, N. J. (33). 1887. F E

Scovell, M. A., Director Kentucky Agricultural Experiment Station, Lexington, Ky. (35). 1887.

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874. F

Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874. C F

Sedgwick, Prof. Wm. T., Massachusetts Institute of Technology, Boston, Mass. (33). 1886. F

See, Horace, 1230 Spruce St., Philadelphia, Pa. (34). 1886. D

Seiler, Carl, M.D., 1346 Spruce St., Philadelphia, Pa. (29). 1882. F B

Sewall, Prof. Henry, Univ. of Mich., Ann Arbor, Mich. (34). 1885. F

Sharples, Stephen P., 13 Broad St., Boston, Mass. (29). 1884. C

Sharpless, Prof. Isaac, Haverford College, Pa. (33). 1888. A

Sheafer, P. W., Pottsville, Pa. (4). 1879. E

Sigsbee, Chas. D., Comd'r U. S. N., U. S. Naval Acad., Annapolis, Md. (28). 1882. D E

Silliman, Prof. Justus M., Lafayette Coll., Easton, Pa. (19). 1874. D E

Skinner, Joseph J., Massachusetts Inst. Technology, Boston, Mass. (23). 1880. B

Smiley, Charles W., U. S. Fish Commission, Washington, D. C. (28). 1883. I

Smith, Prof. Chas. J., 85 Adelbert St., Cleveland, Ohio (32). 1885. A B

Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30). 1882. A B

Smith, Prof. Erastus G., Beloit College, Beloit, Wis. (34). 1887. C

Smith, Prof. Eugene A., University, Ala. (20). 1877. E C

Smith, Prof. Francis H., University of Virginia, Charlottesville, Va. (26). 1880. B A

Smith, John B., National Museum, Washington, D. C. (32). 1884. F

SMITH, QUINTIUS C., M.D., No. 617 Colorado St., Austin, Texas (26). 1881. F

Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875. F

Smith, Dr. Theobald, Bureau of Animal Industry, U. S. Dep't of Agric., Washington, D. C. (35). 1887. F

Smock, Prof. John Conover, New York State Museum, Albany, N. Y. (23). 1879. E

Snow, Prof. F. H., Lawrence, Kan. (29). 1881. F E

Snyder, Henry, B.Sc., Miami Univ., Oxford, Ohio. (30). 1888. B C

Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882. A B

Soule, R. H., Gen. Agent, The Union Switch & Signal Co., Pittsburgh, Pa. (33). 1886. D

Spalding, Volney M., Ann Arbor, Mich. (34). 1886. F

Spencer, Prof. J. William, Prof. of Geology, University of Georgia, Athens, Ga. (28). 1882. E

Springer, Dr. Alfred, Box 621, Cincinnati, Ohio (24). 1880. C

Staley, Cady, LL.D., Pres. Case School of Applied Sciences, Cleveland, Ohio (37). 1888. D

Stearns, R. E. C., care Smithsonian Institution, Washington, D. C. (18). 1874. F

Steiner, Dr. Lewis H., Enoch Pratt Free Library, Baltimore, Md. (7). 1874. I

STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874. E H

Sternberg, George M., Surgeon U. S. A., Johns Hopkins Univ., Baltimore, Md. (24). 1880. F

Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882. B A C

Stevenson, Prof. John J., Univ. of New York, New York, N. Y. (36). 1888.

Stockwell, John N., 1008 Case Avenue, Cleveland, Ohio (18). 1875. A

Stone, George H., Colorado Springs, Col. (29). 1882. E F

Stone, Mrs. Leander, 3417 Indiana Avenue, Chicago, Ill. (22). 1874. F E

Stone, Ormond, Director Leander McCormick Observatory, University of Virginia, Va. (24). 1876. A

Storrs, Henry E., Jacksonville, Ill. (20). 1874. C E

Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881. A

Stowell, Prof. T. B., Cortland, Cortland Co., N. Y. (28). 1885. F

Stringham, Prof. Irving, Univ. of Cal., Berkeley, Cal. (33). 1885. A

Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874. C

Sturtevant, E. Lewis, M.D., Geneva, N. Y. (29). 1882. F

Swift, Lewis, Ph.D., Rochester, N. Y. (29). 1882. A

Tainter, Sumner, 2020 F St. N. W., Washington, D. C. (29). 1881. B D A

Taylor, Thos., M.D., Dep't of Agric., Washington, D. C. (29). 1885. F C

Taylor, William B., Smithsonian Institution, Washington, D. C. (29). 1881. B A

Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874. B

Thomas, Benj. F., Ph.D., State Univ., Columbus, Ohio (29). 1882. B A

Thomson, Elihu, Thomson-Houston Electric Co., Lynn, Mass. (37). 1888. B

Thomson, Wm., M.D., 1426 Walnut St., Philadelphia, Pa. (33). 1885. B

Thurston, Prof. R. H., Sibley College, Cornell University, Ithaca, N. Y. (23). 1875. D

Tittmann, Otto H., U. S. Coast and Geodetic Survey Office, Washington, D. C. (24). 1888. A

Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881. A B D

Todd, Prof. James E., Tabor, Fremont Co., Iowa (22). 1886. E F

Towne, Henry R., Pres. Yale and Towne Manufacturing Co., Stamford, Conn. (33). 1888. D B

Townshend, Prof. N. S., Ohio State Univ., Columbus, Ohio (17). 1881. **F H**
Tracy, Sam'l M., Agricultural College, Miss. (27). 1881. **F**
Trembley, J. B., M.D., 952 8th St., Oakland, Alameda Co., Cal. (17). 1880. **B F**
Trowbridge, Prof. W. P., School of Mines, Columbia College, New York, N. Y. (10). 1874. **D**
True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882. **F**
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882. **H**
Tucker, Willis G., M.D., Albany Med. Coll., Albany, N. Y. (29). 1888. **C**
Tuttle, Prof. Albert H., Univ. of Virginia, Va. (17). 1874. **F**

Uhler, Philip R., 218 W. Hoffman St., Baltimore, Md. (19). 1874. **F E**
Underwood, Prof. Lucien M., cor. Comstock Ave. and Marshall St., Syracuse, N. Y. (33). 1885. **F**
Upham, Warren, 21 Newbury St., Somerville, Mass. (25). 1880. **E**
Upton, Winslow, Brown Univ., Providence, R. I. (29). 1883. **A**

Van der Weyde, P. H., M.D., 286 Duffield St., Brooklyn, N. Y. (17). 1874. **B**
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882. **B C F**
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875. **A**
Vaughn, Dr. Victor C., Ann Arbor, Mich. (34). 1887. **C**
Very, Samuel W., Lieut. Comdr. U. S. N., U. S. S. Santee, Naval Acad., Annapolis, Md. (28). 1886. **A B**
Vining, Edward P., N. Y. & N. E. R. R. Co., 246 Federal St., Boston, Mass. (32). 1887. **H**
Vogdes, A. W., 1st Lt. 5th Art'y U. S. A., The Military Service Inst., Governor's Island, N. Y. (32). 1885. **E F**

Wachsmuth, Charles, 111 Marietta St., Burlington, Iowa (30). 1884. **E F**
Wadsworth, Prof. M. Edward, Ph.D., Director of the Michigan Mining School, State Geologist of Michigan, Houghton, Mich. (23). 1874. **E**
Walcott, Charles D., U. S. Geological Survey, Washington, D. C. (25). 1882. **E F**
Waldo, Leonard, S.D., Lockport, N. Y. (28). 1880. **A**
Wallace, Wm., Ansonia, Conn. (28). 1882.
WALLER, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28). 1883. **F**
Ward, Prof. Henry A., Rochester, N. Y. (18). 1875. **F E H**
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E F**
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874. **F B**
Warder, Prof. Robert B., Howard Univ., Washington, D. C. (19). 1881. **C B**
WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874. **A B**

Warren, Cyrus M., Brookline, Mass. (29). 1882. C
Warren, Dr. Joseph W., 119 Boylston St., Boston, Mass. (31). 1886. F
Warren, Prof. S. Edward, Newton, Mass. (17). 1875. A—I
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875. F
WATSON, PROF. WM., 107 Marlborough St., Boston, Mass. (12). 1884. A
Wead, Prof. Charles K., Hartford, Conn. (23). 1880. B
Webb, Prof. J. Burkitt, Stevens Inst., Hoboken, N. J. (31). 1883. D B A
Weber, Prof. Henry A., Ohio State Univ., Columbus, Ohio. (35). 1888. F
Webster, Prof. N. B., Park Ave., Norfolk, Va. (7). 1874. B C E
Wells, Daniel H., Hartford, Conn. (18). 1875. A I
Wendell, Oliver C., Observatory, Cambridge, Mass. (29). 1886.
Westcott, O. S., Maywood, Cook Co., Ill. (21). 1874. H F A
Weston, Edward, 645 High St., Newark, N. J. (33). 1887. B C D
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Prof. C. Gilbert, 81 Clark St., Chicago, Ill. (18). 1883. C E
Wheeler, Orlando B., 348 N. 18th St., Philadelphia, Pa. (24). 1882. A D
Wheildon, W. W., Box 229, Concord, Mass. (13). 1874. B E
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875. E F
White, Prof. H. C., Univ. of Georgia, Athens, Ga. (29). 1885. C
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882. E
Whiteaves, J. F., Geological Survey, Ottawa, Can. (31). 1887. E F
Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874. E F H
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883. B A
Whitman, Prof. Frank P., Adelbert College, Cleveland, Ohio (33). 1885.
Wilber, G. M., Pine Plains, N. Y. (19). 1874. F H
Wilbur, A. B., Middletown, N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875. F
Wiley, Prof. Harvey W., Dep't of Agric., Washington, D.C. (21). 1874. C
Williams, Benezette, 171 La Salle St., Chicago, Ill. (33). 1887. D
Williams, Charles H., M.D., 7 Otis Place, Boston, Mass. (22). 1874.
Williams, Geo. Huntington, Johns Hopkins Univ., Baltimore, Md. (33). 1886. E
Williams, Henry Shaler, Cornell Univ., Ithaca, N. Y. (18). 1882. E F
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874. H F
Williams, Prof. S. G., Cornell Univ., Ithaca, N. Y. (33). 1885. E
Willson, Prof. Frederick N., Princeton, N. J. (33). 1887. A D
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876. H E
Wilson, H. C., U. S. Naval Observatory, Washington, D. C. (30). 1885. A
Wilson, Joseph M., Room 1036, Drexel Building, Philadelphia, Pa. (33). 1886. D
Wilson, Thomas, U. S. Nat'l Museum, Washington, D. C. (36). 1888. H
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875. E

Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874. **E H**

Winlock, Wm. C., U. S. N. Observ., Washington, D. C. (33). 1885. **A B**

Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881. **D A**

Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.

Woodbury, C. J. H., 31 Milk St., Boston, Mass. (29). 1884. **D**

Woodward, Prof. Calvin M., 1761 Missouri Ave., St. Louis, Mo. (32). 1884. **D A I**

Woodward, R. S., care of U. S. Geol. Survey, Washington, D. C. (33). 1885. **A B D**

Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.

Worthen, W. E., 63 Bleeker St., New York, N. Y. (36). 1888. **D**

Wrampelmeier, Theo. J., San Diego, Cal. (34). 1887. **C**

Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880. **E F**

Wright, Prof. Arthur W., Yale Coll., New Haven, Conn. (14). 1874. **A B**

Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882. **E**

Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875. **E**

Young, A. V. E., Northwestern Univ., Evanston, Ill. (33). 1886. **C B**

Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874. **A B D**

Zentmayer, Joseph, 147 S. Fourth St., Philadelphia, Pa. (29). 1882. **F**

[690 FELLOWS.]

SUMMARY.—PATRONS, 3; MEMBERS, 1271; HONORARY FELLOW, 1; FELLOWS, 689.

APRIL 12, 1889. TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1964.

DECEASED MEMBERS.

[Unless by special vote of the Council, the names of those only who are members of the Association at the *time of their decease* will be included in this list. Information of the date and place of birth and death, to fill blanks in this list, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (28). Died Sept. 25, 1879.

Abert, John James, Washington, D. C. (1). Born in Shepherdstown, Va., Sept. 17, 1788. Died in Washington, D. C., Sept. 27, 1863.

Adams, Charles Baker, Amherst, Mass. (1). Born in Dorchester, Mass., Jan. 11, 1814. Died in St. Thomas, W. I., Jan. 19, 1853.

Adams, Edwin F., Charlestown, Mass. (18).

Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born in Parish of Motier, Switzerland, May 28, 1807. Died in Cambridge, Mass., Dec. 14, 1873.

Ainsworth, J. G., Barry, Mass. (14).

Alexander, Stephen, Princeton, N. J. (1). Born Sept. 1, 1806. Died June 25, 1883.

Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.

Allen, Zachariah, Providence, R. I. (1). Born in Providence, R. I., Sept. 15, 1795. Died March 17, 1882.

Allston, Robert Francis Withers, Georgetown, S. C. (3). Born in All Saints Parish, S. C., April 21, 1801. Died near Georgetown, S. C., April 7, 1864.

Alvord, Benjamin, Washington, D. C. (17). Born in Rutland, Vt., Aug. 18, 1813. Died Oct. 16, 1884.

Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.

Andrews, Ebenezer Baldwin, Lancaster, Ohio (7). Born in Danbury, Conn., April 29, 1821. Died in Lancaster, Ohio, Aug. 14, 1880.

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Appleton, Nathan, Boston, Mass. (1). Born in New Ipswich, N. H., Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Ashburner, Wm., San Francisco, Cal. (29). Born in Stockbridge, Mass., March, 1831. Died in San Francisco, Cal., April 20, 1887.

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Aufrecht, Louis, Cincinnati, Ohio (30).

Bache, Alexander Dallas, Washington, D. C. (1). Born in Philadelphia, Pa., July 19, 1806. Died at Newport, R. I., Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob Whitman, West Point, N. Y. (1). Born in Auburn, Mass., April 29, 1811. Died in West Point, N. Y., Feb. 26, 1857.

Baird, Spencer Fullerton, Washington, D. C. (1). Born in Reading, Pa., Feb. 3, 1823. Died in Wood's Holl, Mass., Aug. 19, 1887.

Bardwell, F. W., Lawrence, Kan. (13). Died in 1878.

Barnard, John Gross, New York, N. Y. (14). Born in Sheffield, Mass., May 19, 1815. Died in Detroit, Mich., May 14, 1882.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Barry, Redmond, Melbourne, Australia (25).

Bassett, Daniel A., Los Angeles, Cal. (29). Born Dec. 8, 1819. Died May 26, 1887.

Bassnett, Thomas, Jacksonville, Fla. (8). Born 1807. Died in Jacksonville, Fla., Feb. 16, 1886.

Bayne, Herbert Andrew, Kingston, Ont., Can. (29). Born in London, derry, Nova Scotia, Aug. 16, 1846. Died in Pictou, Can., Sept. 16, 1886.

Beach, J. Watson, Hartford, Conn. (23). Born Dec. 28, 1823. Died Mar. 16, 1887.

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis Caleb, New Brunswick, N. J. (1). Born in Schenectady, N. Y., Oct. 4, 1798. Died April 20, 1853.

Beck, Theodoric Romeyn, Albany, N. Y. (1). Born in Schenectady, N. Y., Aug. 11, 1791. Died in Utica, N. Y., Nov. 19, 1855.

Beckwith, Henry C., Coleman's Station, N. Y. (29). Died July 12, 1885.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.

Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.

Benedict, George Wyllys, Burlington, Vt. (16). Born Jan. 11, 1796. Died Sept. 23, 1871.

Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19, 1877.

Binney, Amos, Boston, Mass. (1). Born in Boston, Mass., Oct. 18, 1803. Died in Rome, Feb. 18, 1847.

Binney, John, Boston, Mass. (3).

Blackie, Geo. S., Nashville, Tenn. (26).

Blair, Henry W., Washington, D. C. (26). Died Dec. 15, 1884.

Blake, Eli Whitney, New Haven, Conn. (1). Born Jan. 27, 1795. Died Aug. 18, 1886.

Blake, Homer Crane, New York, N. Y. (28). Born in Cleveland, Ohio, Feb. 1, 1822. Died in New York, N. Y., Jan. 20, 1880.

Blanding, William, ——, R. I. (1).

Blatchford, Thomas W., Troy, N. Y. (6).

Blatchley, Miss S. L., New Haven, Conn. (19). Died March 13, 1873.

Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.

Bomford, George, Washington, D. C. (1). Born in New York, 1780. Died in Boston, Mass., March 25, 1848.

Bowles, Miss Margaretta, Columbia, Tenn. (26). Died July, 1887.

Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.
Briggs, Robert, Philadelphia, Pa. (29). Born May 18, 1822. Died July 24, 1882.
Brigham, Charles Henry, Ann Arbor, Mich. (17). Born in Boston, Mass., July 27, 1820. Died Feb. 19, 1879.
Brown, Andrew, Natchez, Miss. (1).
Brown, Horace, Salem, Mass. (27). Died in July, 1883.
Bull, John, Washington, D. C. (31). Born Aug. 1, 1819. Died June 9, 1884.
Burbank, L. S., Woburn, Mass. (18).
Burke, Joseph Chester, Middletown, Conn. (29). Died in 1885.
Burnap, George Washington, Baltimore, Md. (12). Born in Merrimack, N. H., Nov. 30, 1802. Died in Philadelphia, Pa., Sept. 8, 1859.
Burnett, Waldo Irving, Boston, Mass. (1). Born in Southborough, Mass., July 12, 1828. Died in Boston, Mass., July 1, 1854.
Butler, Thomas Belden, Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, Frederick A., New York, N. Y. (27). Died in 1879.
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5, 1880.
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died in Providence, R. I., Jan. 8, 1877.
Chadbourne, Paul Ansel, Amherst, Mass. (10). Born in North Berwick, Me., Oct. 21, 1823. Died Feb. 23, 1883.
Chapman, Nathaniel, Philadelphia, Pa. (1). Born in Alexandria Co., Va., May 28, 1780. Died July 1, 1853.
Chase, Pliny Earle, Haverford College, Pa. (18). Born in Worcester, Mass., Aug. 18, 1820.
Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.
Cheesman, Louis Montgomery, Hartford, Conn. (32). Born in 1858. Died in Jan., 1885.
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29). Died in 1882.
Chevreul, Michel Eugène, Paris, France (35). Born in Angiers, France, Aug. 31, 1786. Died in April, 1889.
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.
Clark, Henry James, Cambridge, Mass. (13). Born in Easton, Mass., June 22, 1826. Died in Amherst, Mass., July 1, 1873.

Clark, Joseph, Cincinnati, Ohio (5).

Clark, Patrick, Rahway, N. J. (33). Died March 5, 1887.

Clarke, A. B., Holyoke, Mass. (18).

Cleaveland, C. H., Cincinnati, Ohio (9).

Cleveland, A. B., Cambridge, Mass. (2).

Coffin, James Henry, Easton, Pa. (1). Born in Northampton, Mass., Sept. 6, 1806. Died Feb. 6, 1873.

Cole, Frederick, Montreal, Can. (31). Died in 1887.

Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.

Coleman, Henry, Boston, Mass. (1).

Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.

Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in New Jersey, June, 21, 1803. Died Aug 9, 1877.

Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.

Cooper, William, Hoboken, N. J. (9). Died in 1864.

Copes, Joseph S., New Orleans, La. (11). Born Dec. 9, 1811. Died March 1, 1885.

Corning, Erastus, Albany, N. Y. (6). Born in Norwich, Conn., Dec. 14, 1794. Died April 9, 1872.

Costin, M. P., Fordham, N. Y. (30). Died June 8, 1884.

Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.

Cramp, John Mockett, Wolfville, N. S. (11). Born in Kent, England, July 25, 1796. Died Dec. 6, 1881.

Crehore, John D., Cleveland, Ohio (24).

Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.

Crocker, Miss Lucretia, Boston, Mass. (29). Died in 1886.

Crosby, Alpheus, Salem, Mass. (10). Born in Sandwich, N. H., Oct. 13, 1810. Died April 17, 1874.

Crosby, Thomas Russell, Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.

Croswell, Edwin, Albany, N. Y. (6). Born in Catskill, N. Y., May 29, 1797. Died June 13, 1871.

Crow, Wayman, St. Louis, Mo. (27). Born March 7, 1808. Died May 10, 1885.

Curry, W. F., Geneva, N. Y. (11).

Curtis, Josiah, Washington, D. C. (18). Died Aug. 1, 1883.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.

Davenport, H. W., Washington, D. C. (30).

Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.

Dean, Amos, Albany, N. Y. (6). Born in Barnard, Vt., Jan. 16, 1803. Died Jan. 26, 1868.

Dearborn, George H. A. S., Roxbury, Mass. (1).

Dekay, James Ellsworth, New York, N. Y. (1). Born in New York, 1792. Died Nov. 21, 1851.

Delano, Joseph C., New Bedford, Mass. (5). Born Jan. 9, 1796. Died Oct. 16, 1886.

DeLaski, John, Carver's Harbor, Me. (18).

Devereux, John Henry, Cleveland, Ohio (18). Born in Boston, Mass., April 5, 1832. Died in Cleveland, Ohio, March 17, 1886.

Dewey, Chester, Rochester, N. Y. (1). Born in Sheffield, Mass., Oct. 25, 1781. Died Dec. 15, 1867.

Dexter, G. M., Boston, Mass. (11).

Dillingham, W. A. P., Augusta, Me. (17).

Dimmick, L. N., Santa Barbara, Cal. (29). Died May 31, 1884.

Dinwiddie, Hardaway H., College Station, Texas (32). Died Dec. 11, 1887.

Dixwell, Geo. B., Boston, Mass. (29). Died April, 1885.

Doggett, George Newell, Chicago, Ill. (33). Born in Chicago, Ill., Dec. 19, 1858. Died in Fredericksburg, Va., Jan. 15, 1887.

Doggett, Mrs. Kate Newell, Chicago, Ill. (17). Born in Castleton, Vt., Nov. 5, 1828. Died in Havana, Cuba, March 13, 1884.

Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.

Doolittle, L., Lenoxville, C. E. (11). Died in 1862.

Dorr, Ebenezer Pearson, Buffalo, N. Y. (25). Born in Hartford, Vt. Died in Buffalo, N. Y., April 29, 1882.

Draper, Henry, New York, N. Y. (28). Born in New York, N. Y., March 7, 1837. Died Nov. 20, 1882.

Ducatel, Julius Timoleon, Baltimore, Md. (1). Born in Baltimore, Md., June 6, 1798. Died April 25, 1849.

Duffield, George, Detroit, Mich. (10). Born in Strasburg, Pa., July 4, 1794. Died in Detroit, Mich., June 26, 1869.

Dumont, A. H., Newport, R. I. (14).

Dun, Walter Angus, Cincinnati, Ohio (31). Born March 1, 1857. Died Nov. 7, 1887.

Duncan, Lucius C., New Orleans, La. (10). Born in 1801. Died Aug. 9, 1855.

Dunn, R. P., Providence, R. I. (14).

Eads, James Buchanan, New York, N. Y. (27). Born May 23, 1820. Died March 8, 1887.

Easton, Norman, Fall River, Mass. (14). Died Dec. 21, 1872.

Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.

Elliott, Ezekiel Brown, Washington, D. C. (10). Born July 16, 1823. Died May 24, 1888.

Elsberg, Louis, New York, N. Y. (28). Born in Iserlohn, Prussia, April 2, 1836. Died in New York, N. Y., Feb. 19, 1885.

Elwyn, Alfred Langdon, Philadelphia, Pa. (1). Born in Portsmouth, N. H., July 9, 1804. Died in Philadelphia, Pa., March 15, 1884.

Ely, Charles Arthur, Elyria, Ohio (4).

Emerson, Geo. Barrell, Boston, Mass. (1). Born in Kennebunk, Me., Sept. 12, 1797. Died March 14, 1881.

Emmons, Ebenezer, Williamstown, Mass. (1). Born in Middlefield, Mass., May 16, 1799. Died October 1, 1863.

Engelmann, George, St. Louis, Mo. (1). Born in Frankfort-on-the Main, Germany, Feb. 2, 1809. Died Feb. 4, 1844.

Engstrom, A. B., Burlington, N. J. (1).

Eustis, Henry Lawrence, Cambridge, Mass. (2). Born Feb. 1, 1819. Died Jan. 11, 1885.

Everett, Edward, Boston, Mass. (2). Born in Dorchester, Mass., April 11, 1794. Died in Boston, Mass., Jan. 15, 1865.

Ewing, Thomas, Lancaster, Ohio (5). Born in Ohio Co., Va., Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.

Farquharson, Robert James, Des Moines, Iowa (24). Born July 15, 1824. Died Sept. 6, 1884.

Ferris, Isaac, New York, N. Y. (6). Born in New York, Oct. 9, 1798. Died in Roselle, N. J., June 16, 1873.

Feuchtwanger, Lewis, New York, N. Y. (11). Born in Fürth, Bavaria, Jan. 11, 1805. Died in New York, N. Y., June 25, 1876.

Fillmore, Millard, Buffalo, N. Y. (7). Born in New York, Jan. 7, 1800. Died March 8, 1874.

Fisher, Mark, Trenton, N. J. (10).

Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.

Fitch, O. H., Ashtabula, Ohio (7). Born in 1803. Died Sept. 17, 1882.

Foote, Herbert Carrington, Cleveland, Ohio (35). Born in 1852. Died Aug. 24, 1888.

Forbush, E. B., Buffalo, N. Y. (15).

Force, Peter, Washington, D. C. (4). Born in New Jersey, Nov. 26, 1790. Died in Washington, D. C., Jan. 23, 1868.

Ford, A. C., Nashville, Tenn. (26).

Forshey, Caleb Goldsmith, New Orleans, La. (21). Born in Somerset Co., Pa., July 18, 1812. Died in Carrollton, La., July 25, 1881.

Foster, John Wells, Chicago, Ill. (1). Born in Brimfield, Mass., March 4, 1815. Died in Chicago, Ill., June 29, 1873.

Foucon, Felix, Madison, Wis. (18).

Fowle, Wm. Bentley, Boston, Mass. (1). Born in Boston, Mass., Oct. 17 1795. Died Feb. 6, 1865.

Fox, Charles, Grosse Ile, Mich. (7).

Frazer, John Fries, Phila., Pa. (1). Born July 8, 1812. Died Oct. 12, 1872.

Freeman, Spencer Hedden, Cleveland, Ohio (29). Born Oct. 3, 1855. Died Feb. 2, 1886.

French, John William, West Point, N. Y. (11). Born in Connecticut, about 1810. Died in West Point, N. Y., July 8, 1871.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.

Gavit, John E., New York, N. Y. (1). Born in New York, Oct. 29, 1819. Died in Stockbridge, Mass., Aug. 25, 1874.

Gay, Martin, Boston, Mass. (1). Born in 1804. Died Jan. 12, 1850.

Gibbon, J. H., Charlotte, N. C. (8).
Gillespie, William Mitchell, Schenectady, N. Y. (10). Born in New York, N. Y., 1816. Died in New York, Jan. 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus Addison, Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, Benjamin Apthorp, Boston, Mass. (2). Born in Lancaster, Mass., June 15, 1787. Died Oct. 24, 1859.
Graham, James D., Washington, D. C. (1). Born in Virginia, 1799. Died in Boston, Mass., Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in Townshend, Vt., Feb. 21, 1808. Died in Brooklyn, N. Y., March 10, 1860.
Gray, Asa, Cambridge, Mass. (1). Born in Paris, N. Y., Nov. 18, 1810. Died in Cambridge, Mass., Jan. 30, 1888.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20). Died in Feb., 1874.
Griffith, Robert Eglesfield, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Feb. 13, 1798. Died June 26, 1854.
Griswold, John Augustus, Troy, N. Y. (19). Born Nov. 11, 1818. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).
Guyot, Arnold, Princeton, N. J. (1). Born Sept. 5, 1809. Died Feb. 8, 1884.

Habel, Louis, Northfield, Vt. (34).
Hackley, Charles William, New York, N. Y. (4). Born in Herkimer Co., N. Y., March 9, 1809. Died in New York, N. Y., January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, Samuel Stehman, Chickies, Pa. (1). Born Aug. 12, 1812. Died Sept. 10, 1880.
Hale, Enoch, Boston, Mass. (1). Born in Westhampton, Mass., Jan. 29, 1790. Died in Boston, Mass., Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Harding, Myron H., Lawrenceburg, Ind. (30.) Died Sept., 1885.
Hare, Robert, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Jan. 17, 1781. Died in Philadelphia, May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Sept. 19, 1796. Died in New Orleans, La., Sept. 30, 1843.
Harris, Thaddeus William, Cambridge, Mass. (1). Born in Dorchester, Mass., Nov. 12, 1795. Died in Cambridge, Mass., Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Benjamin Franklin, Wallingford, Conn. (11). Born April 19, 1811. Died April 23, 1886.

Harrison, Jos., jr., Philadelphia, Pa. (12). Born in Philadelphia, Pa., Sept. 20, 1810. Died in Philadelphia, March 27, 1874.

Hart, Simeon, Farmington, Conn. (1). Born Nov. 17, 1795. Died April 20, 1853.

Hartt, Charles Frederick, Ithaca, N. Y. (18). Born in Nova Scotia, Aug. 20, 1840. Died March 18, 1878.

Haven, Joseph, Chicago, Ill. (17). Born in Dennis, Mass., Jan. 4, 1816. Died May 23, 1874.

Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.

Hayden, Ferdinand Vandeveer, Philadelphia, Pa. (29). Born in Westfield, Mass., Sept. 7, 1829. Died Dec. 22, 1887.

Hayden, Horace H., Baltimore, Md. (1). Born in Winsor, Conn., Oct. 13, 1769. Died in Baltimore, Md., Jan. 26, 1844.

Hayes, George E., Buffalo, N. Y. (15).

Hayward, James, Boston, Mass. (1). Born in Concord, Mass., June 12 1786. Died in Boston, Mass., July 27, 1866.

Hazen, William Babcock, Washington, D. C. (30). Born in Hartford, Vt., Sept. 27, 1830. Died Jan. 16, 1887.

Hedrick, Benjamin Sherwood, Washington, D. C. (19). Born in 1826. Died Sept. 2, 1886.

Heighway, A. E., Cincinnati, Ohio (29). Born Dec. 26, 1820. Died Jan. 24, 1888.

Hempstead, G. S. B., Portsmouth, Ohio (29). Born in 1795. Died July 9, 1883.

Henry, Joseph, Washington, D. C. (1). Born in Albany, N. Y., Dec. 17 1797. Died May 13, 1878.

Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.

Hicks, William C., New York, N. Y. (34). Died in 1885.

Hilgard, Theodore Charles, St. Louis, Mo. (17). Born in Zweibrücken, Germany, Feb. 28, 1828. Died March 5, 1875.

Hill, Walter N., Chester, Pa. (29). Born Apr. 15, 1846. Died March 29, 1884.

Hincks, William, Toronto, C. W. (11). Born in 1801. Died July, 1871.

Hitchcock, Edward, Amherst, Mass. (1). Born in Deerfield, Mass., May 24, 1793. Died Feb. 27, 1864.

Hoadley, John Chipman, Boston, Mass. (29). Born Dec. 10, 1818. Died Oct. 21, 1886.

Hodgson, W. B., Savannah, Ga. (10). Born 1815.

Holbrook, John Edwards, Charleston, S. C. (1). Born in Beaufort, S. C., Dec. 30, 1796. Died in Norfolk, Mass., Sept. 8, 1871.

Holman, Mrs. S. W., Boston, Mass. (29). Died May 5, 1885.

Homes, Henry A., Albany, N. Y. (11). Born in 1812. Died Nov. 3, 1887.

Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.

Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.

Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.

Hopkins, Wm., Lima, N. Y. (5). Died in March, 1867.

Hoppock, Albert E., Hastings-on-Hudson, N. Y. (29).

Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.

Horton, William, Craigville, N. Y. (1).

Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.

Hough, Franklin Benjamin, Lowville, N. Y. (4). Born in Martinsville, N. Y., July 20, 1822. Died June 6, 1885.

Houghton, Douglas, Detroit, Mich. (1). Born in Troy, N. Y., Sept. 21, 1809. Died Oct. 13, 1845.

Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.

Howland, Edward Perry, Washington, D. C. (29). Born in Ledyard, N. Y., July 20, 1825. Died in Harrisburg, Pa., Sept. 12, 1888.

Howland, Theodore, Buffalo, N. Y. (15).

Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.

Hunt, Edward Bissell, Washington, D. C. (2). Born in Livingston Co., N. Y., June 15, 1822. Died in Brooklyn, N. Y., Oct. 2, 1863.

Hunt, Freeman, New York, N. Y. (11). Born in Quincy, Mass., March 21, 1804. Died in Brooklyn, N. Y., March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.

Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles Thomas, Boston, Mass. (1). Born in Plymouth, Mass., June 21, 1805. Died Aug. 28, 1880.

James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.

Johnson, Walter Rogers, Washington, D. C. (1). Born in Leominster, Mass., June 21, 1794. Died April 26, 1852.

Johnson, William Schuyler, Washington, D. C. (31). Born Sept. 20, 1859. Died Oct. 6, 1883.

Jones, Catesby A. R., Washington, D. C. (8).

Jones, Henry A., Portland, Me. (29). Died Sept. 3, 1883.

Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).

Keely, George W., Waterville, Me. (1). Died in 1878.

Keep, N. C., Boston, Mass. (18). Died in March, 1875.

Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.

Kerr, Washington Caruthers, Raleigh, N. C. (10). Born May 24, 1827. Died Aug. 9, 1885.

Kidder, Henry Purkitt, Boston, Mass. (29). Born Jan. 8, 1823. Died Jan. 28, 1886.

King, Mitchell, Charleston, S. C. (3). Born in Scotland, June 8, 1783. Died Nov. 12, 1862.

Kirkpatrick, James A., Philadelphia, Pa. (7). Died June 3, 1886.

Kite, Thomas, Cincinnati, Ohio (5). Died Feb. 6, 1884.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase Allen, Milwaukee, Wis. (3). Born in Palmyra, N. Y. March 7, 1811. Died in Oconomowoc, Wis., Sept. 14, 1875.
Larkin, Ethan Pendleton, Alfred Centre, N. Y. (33). Born Sept. 20, 1829. Died Aug. 23, 1887.
LaRoche, Réné, Philadelphia, Pa. (12). Born in Philadelphia, Pa., 1795. Died in Philadelphia, Dec., 1872.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died Jan. 31, 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lawrence, Edward, Charlestown, Mass. (18). Born June, 1810. Died Oct. 17, 1885.
Lea, Isaac, Philadelphia, Pa. (1). Born in Wilmington, Del., March 4, 1792. Died Dec. 8, 1886.
Le Conte, John Lawrence, Philadelphia, Pa. (1). Born in New York, May 13, 1825. Died Nov. 15, 1883.
Lederer, Baron von, Washington, D. C. (1).
Lewis, Henry Carvill, Philadelphia, Pa. (26). Born in Philadelphia, Pa., Nov. 16, 1853. Died in Manchester, England, July 21, 1888.
Libbey, Joseph. Georgetown, D. C. (31). Died July 20, 1886.
Lieber, Oscar Montgomery, Columbia, S. C. (8). Born Sept. 8, 1830. Died June 27, 1862.
Lincklaen, Ledyard, Cazenovia, N. Y. (1). Died April 25, 1864.
Linsley, James Harvey, Stafford, Conn. (1). Born in Northford, Conn., May 5, 1787. Died in Stratford, Conn., Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.
Logan, William Edmond, Montreal, Canada (1). Born in Montreal, Canada, April 23, 1798. Died in Wales, June 22, 1875.
Loiseau, Emile F., Brussels, Belgium (33). Died April 30, 1886.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lowrie, J. R., Warriorsmark, Pa. (29). Died Dec. 10, 1885.
Lull, Edward Phelps, Washington, D. C. (28). Born Feb. 20, 1836. Died March 5, 1887.
Lyford, Moses, Springfield, Mass. (22). Died Aug. 4, 1887.
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

M'Conihe, Isaac, Troy, N. Y. (5).
McCutchen, A. R., Atlanta, Ga. (25). Died Nov. 21, 1887.
McFadden, Thomas, Westerville, Ohio (30). Born Nov. 9, 1825. Died Nov. 9, 1883.

MacGregor, Donald, Houston, Texas (33). Died in Oct., 1887.
McLachlan, J. S., Montreal, Can. (31).
McMahon, Mathew, Albany, N. Y. (11).
Maack, G. A., Cambridge, Mass. (18). Died in Aug., 1873.
Macfarlane, James, Towanda, Pa. (29). Died in 1885.
Mahan, Dennis Hart, West Point, N. Y. (9). Born in New York, N. Y., April 2, 1802. Died in New York, Sept. 16, 1871.
Marler, George L., Montreal, Can. (31).
Marsh, Dexter, Greenfield, Mass. (1). Born in Montague, Mass., Aug. 22, 1806. Died in Greenfield, Mass., April 2, 1853.
Marsh, James E., Roxbury, Mass. (10).
Martin, Benjamin Nichols, New York, N. Y. (23). Born in Mount Holly, N. J., Oct. 20, 1816. Died in New York, N. Y., Dec. 26, 1883.
Mather, William Williams, Columbus, Ohio (1). Born in Brooklyn, Conn., May 24, 1804. Died in Columbus, Ohio, Feb. 27, 1859.
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
Meade, George Gordon, Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, Fielding Bradford, Washington, D. C. (6). Born Dec. 10, 1817. Died Dec. 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born Aug. 14, 1805. Died Oct. 24, 1880.
Mitchel, Ormsby MacKnight, Cincinnati, Ohio (3). Born in Union Co., Ky., July 28, 1810. Died in Beaufort, S. C., Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Born in Nantucket, Mass., Dec. 20, 1791. Died in Poughkeepsie, N. Y., April 19, 1868.
Mitchell, Wm. H., Florence, Ala. (17).
Monroe, Nathan, Bradford, Mass. (6). Born in Minot, Me., May 16, 1804. Died in Bradford, Mass., July 8, 1866.
Monroe, William, Concord, Mass. (18). Died April 27, 1877.
Morgan, Lewis Henry, Rochester, N. Y. (10). Born near Aurora, N. Y., Nov. 21, 1818. Died Dec. 17, 1881.
Morgan, Mrs. Mary E., Rochester, N. Y. (31). Died in 1884.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel George, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Jan. 26, 1799. Died in Philadelphia, May 15, 1851.
Mudge, Benjamin Franklin, Manhattan, Kansas (25). Born in Orrington, Me., Aug. 11, 1817. Died Nov. 21, 1879.
Muir, William, Montreal, Can. (31). Died July, 1885.
Mussey, William Heberdom, Cincinnati, Ohio (30). Born Sept. 30, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).

Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.

Nichols, William Ripley, Boston, Mass. (18). Born April 30, 1847. Died July 14, 1886.

Nicholson, Thomas, New Orleans, La. (21).

Nicollet, Jean Nicholas, Washington, D. C. (1). Born in Savoy, France, July 24, 1786. Died in Washington, D. C., Sept. 11, 1843.

Norton, John Pitkin, New Haven, Conn. (1). Born July 19, 1822. Died Sept. 5, 1852.

Norton, William Augustus, New Haven, Conn. (6). Born in East Bloomfield, N. Y., Oct. 25, 1810. Died Sept. 21, 1883.

Noyes, James Oscar, New Orleans, La. (21). Born in Niles, N. Y., June 14, 1829. Died in New Orleans, La., Sept. 11, 1872.

Nutt, Cyrus, Bloomington, Ind. (20). Born in Trumbull Co., Ohio, Sept. 4, 1814. Died in Bloomington, Aug. 23, 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.

Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.

Ogden, William Butler, High Bridge, N. Y. (17). Born in New York, N. Y., 1805. Died in New York, Aug. 3, 1877.

Oliver, Miss Mary E., Ithaca, N. Y. (20).

Olmsted, Alexander Fisher, New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.

Olmsted, Denison, New Haven, Conn. (1). Born in East Hartford, Conn., June 18, 1791. Died in New Haven, Conn., May 13, 1859.

Olmsted, Denison, Jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.

Orton, James, Poughkeepsie, N. Y. (18). Born in Seneca Falls, N. Y., April 21, 1830. Died in Peru, S. A., Sept. 24, 1877.

Osbun, Isaac J., Salem, Mass. (29).

Otis, George Alexander, Washington, D. C. (10). Born in Boston, Mass., Nov. 12, 1830. Died Feb. 23, 1881.

Packer, Harry E., Mauch Chunk, Pa. (30). Died Feb. 1, 1884.

Painter, Jacob, Lima, Pa. (23). Died in 1876.

Painter, Minshall, Lima, Pa. (7).

Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.

Parkman, Samuel, Boston, Mass. (1). Born in 1816. Died Dec. 15, 1854.

Parsons, Henry Betts, New York, N. Y. (30). Born Nov. 20, 1855. Died Aug. 21, 1885.

Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.

Peirce, Benjamin Osgood, Beverly, Mass. (18). Born in Beverly, Sept. 26, 1812. Died in Beverly, Nov. 12, 1883.

Peirce, Benjamin, Cambridge, Mass. (1). Born in Salem, Mass., April 4, 1809. Died in Cambridge, Mass., Oct. 6, 1880.

Perch, Bernard, Frankford, Pa. (35). Born in 1850. Died in 1887.

Perkins, George Roberts, Utica, N. Y. (1). Born in Otsego Co., N. Y., May 3, 1812. Died in New Hartford, N. Y., Aug. 22, 1876.
Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16). Born in 1820. Died Oct. 3, 1872.
Perry, Matthew Calbraith, New York, N. Y. (10). Born in South Kingston, R. I., 1795. Died in New York, March 4, 1858.
Phelps, Mrs. Almira Hart Lincoln, Baltimore, Md. (13). Born in Berlin, Conn., July 15, 1793. Died in Berlin, July 15, 1884.
Phillips, John C., Boston, Mass. (29). Born in 1839. Died March 1, 1885.
Piggot, A. Snowden, Baltimore, Md. (10).
Pim, Bedford Clapperton Trevelyan, London, Eng. (33). Born in England, June 12, 1826. Died Oct., 1886.
Platt, W. G., Philadelphia, Pa. (32). Died Nov., 1885.
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles Alexander, St. Louis, Mo. (12). Born in Huntsville, Ala., March, 15, 1818. Died in Paris, Mo., July 6, 1870.
Porter, John Addison, New Haven, Conn. (14). Born in Catskill, N. Y., March 15, 1822. Died in New Haven, Conn., Aug. 25, 1866.
Potter, Stephen H., Hamilton, Ohio (30). Born Nov. 10, 1812. Died Dec. 9, 1883.
Pourtalès, Louis François de, Cambridge, Mass. (1). Born March 4, 1824. Died July 19, 1880.
Pruyn, John Van Schaick Lansing, Albany, N. Y. (1). Born in Albany, N. Y. June 22, 1811. Died in Clifton Springs, N. Y., Nov. 21, 1877.
Pugh, Evan, Centre Co., Pa. (14). Born Feb. 29, 1828. Died April 29, 1864.
Pulsifer, Sidney, Philadelphia, Pa. (21). Died March 24, 1884.
Putnam, Mrs. Frederick Ward, Cambridge, Mass. (19). Born in Charlestown, Mass., Dec. 29, 1838. Died in Cambridge, Mass., March 10, 1879.
Putnam, J. Duncan, Davenport, Iowa (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born near Middletown, Conn., March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rochester, Thomas Fortescue, Buffalo, N. Y. (35). Born Oct. 8, 1823. Died May 24, 1887.
Rockwell, John Arnold, Norwich, Conn. (10). Born in Norwich, Conn., August 27, 1803. Died in Washington, D. C., February 10, 1861.
Roeder, F. A., Cincinnati, Ohio (30).
Rogers, Henry Darwin, Glasgow, Scotland (1). Born in Philadelphia, Pa., Aug. 1, 1808. Died in Glasgow, Scotland, May 29, 1866.

Rogers, James Blythe, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Feb. 11, 1802. Died in Philadelphia, June 15, 1852.

Rogers, Robert Empie, Philadelphia, Pa. (18). Born in Baltimore, Md., March 29, 1813. Died Sept. 6, 1884.

Rogers, William Barton, Boston, Mass. (1). Born in Philadelphia, Pa., Dec. 7, 1804. Died in Philadelphia, May 30, 1882.

Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born in Bethlehem, N. Y., Dec. 22, 1811. Died August 6, 1877.

Sanders, Benjamin D., Wellsburg, W. Va. (19).

Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.

Schley, William, New York, N. Y. (28). Died in 1882.

Scott, Joseph, Dunham, C. E. (11). Died in 1865.

Seaman, Ezra Champion, Ann Arbor, Mich. (20). Born Oct. 14, 1805. Died July 15, 1880.

Senter, Harvey S., Aledo, Ill. (20). Died in 1875.

Seward, William Henry, Auburn, N. Y. (1). Born in Florida, N. Y., May 16, 1801. Died in Auburn, N. Y., Oct. 10, 1872.

Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.

Sherwin, Thomas, Dedham, Mass. (11). Born in Westmoreland, N. H., March 26, 1799. Died in Dedham, Mass., July 23, 1869.

Sill, Elisha N., Cuyahoga Falls, Ohio (6). Born in 1801. Died April 26, 1888.

Silliman, Benjamin, New Haven, Conn. (1). Born in North Stratford, Conn., August 8, 1779. Died in New Haven, Conn., Nov. 22, 1864.

Silliman, Benjamin, New Haven, Conn. (1). Born in New Haven, Conn., Dec. 4, 1816. Died Jan. 14, 1885.

Skinner, John B., Buffalo, N. Y. (15). Died in 1871.

Slack, J. H., Philadelphia, Pa. (12).

Smith, Charles A., St. Louis, Mo. (27). Died in 1884.

Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec. 26, 1880.

Smith, Mrs. Erminnie Adelle, Jersey City, N. J. (25). Born April 26, 1836. Died June 9, 1886.

Smith, John Lawrence, Louisville, Ky. (1). Born near Charleston, S. C., Dec. 17, 1818. Died Oct. 12, 1883.

Smith, J. V., Cincinnati, Ohio (5).

Smith, James Young, Providence, R. I. (9). Born in Groton, Conn., Sept. 15, 1809. Died March 26, 1876.

Smith, Lyndon Arnold, Newark, N. J. (9). Born in Haverhill, N. H., November 11, 1795. Died in Newark, N. J., December 15, 1865.

Snell, Ebenezer Strong, Amherst, Mass. (2). Born in North Brookfield, Mass., October 7, 1801. Died in Amherst, Mass., Sept., 1877.

Sparks, Jared, Cambridge, Mass. (2). Born in Willington, Conn., May 10, 1819. Died in Cambridge, Mass., March 14, 1866.

Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.

Squier, Ephraim George, New York, N. Y. (18). Born in Bethlehem, N. Y., June 17, 1821. Died in Brooklyn, N. Y., April 17, 1888.

Stearns, Josiah A., Boston, Mass. (29).

Stearns, Silas, Pensacola, Fla. (28). Died Aug. 2, 1888.

Steele, Joel Dorman, Elmira, N. Y. (33). Born in Lima, N. Y., May 14, 1836. Died May 25, 1886.

Stevenson, James, Washington, D. C. (29). Born Dec. 24, 1840. Died July 23, 1888.

Stimpson, William, Chicago, Ill. (12). Born Feb. 14, 1832. Died May 26, 1872.

Stone, Leander, Chicago, Ill. (32). Died April 2, 1888.

Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.

St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.

Straight, H. H., Chicago, Ill. (25). Died Nov. 17, 1886.

Sullivan, Algernon Sidney, New York, N. Y. (36). Born April 5, 1826. Died Dec. 4, 1887.

Sullivant, William Starling, Columbus, Ohio (7). Born near Columbus, O., Jan 15, 1803. Died in Columbus, O., April 30, 1873.

Sutton, George, Aurora, Ind. (20.) Died June 13, 1886.

Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born in Stamford, N. Y., Jan. 20, 1778. Died in New York, N. Y., Oct. 3, 1853.

Taylor, Arthur F., Cleveland, Ohio (29). Born Dec. 10, 1853. Died June 28, 1883.

Taylor, Richard Cowling, Philadelphia, Pa. (1). Born in England, Jan. 18, 1789. Died in Philadelphia, Pa., November 26, 1851.

Taylor, Robert N., Tollesboro, Ky. (37). Died Aug. 13, 1888.

Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.

Teschemacher, James Englehert, Boston, Mass. (1). Born in Nottingham, England, June 11, 1790. Died near Boston, Nov. 9, 1853.

Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.

Thompson, Alexander, Aurora, N. Y. (1).

Thompson, Charles Oliver, Terre Haute, Ind. (29). Born in East Windsor Hill, Conn., Sept. 25, 1835. Died in Terre Haute, Ind., March 17, 1885.

Thompson, Zadock, Burlington, Vt. (1). Born in Bridgewater, Vt., May 23, 1796. Died in Burlington, Vt, Jan 19, 1856.

Thomson, Henry R., Crawfordsville, Ind. (30). Died in 1884.

Thurber, Isaac, Providence, R. I. (9).

Tillman, Samuel Dyer, Jersey City, N. J. (15). Born April, 1815. Died Sept. 4, 1875.

Tobin, Thomas W., Louisville, Ky. (30). Died Aug. 4, 1883.

Todd, Albert, St. Louis, Mo. (27). Born March 4, 1813. Died April 30, 1885.

Tolderoy, James B., Fredericton, N. B. (11).

Torrey, John, New York, N. Y. (1). Born in New York, N. Y., Aug. 15, 1796. Died in New York, March 10, 1873.

Torrey, Joseph, Burlington, Vt. (2). Born in Rowley, Mass., Feb. 2, 1797. Died in Burlington, Vt., Nov. 26, 1867.

Totten, Joseph Gilbert, Washington, D. C. (1). Born in New Haven, Conn., August 23, 1788. Died in Washington, D. C., April 22, 1864.

Townsend, Howard, Albany, N. Y. (10). Born Nov. 22, 1823. Died Jan. 6, 1867.

Townsend, John Kirk, Philadelphia, Pa. (1). Born Aug. 10, 1809. Died Feb. 16, 1851.

Townsend, Robert, Albany, N. Y. (9). Born 1799. Died Aug. 15, 1866.

Troost, Gerard, Nashville, Tenn. (1). Born in Bois-le-Duc, Holland, March 15, 1776. Died in Nashville, Tenn., Aug. 14, 1850.

Tuomey, Michael, Tuscaloosa, Ala. (1). Born in Ireland, September 29, 1805. Died in Tuscaloosa, Ala., March 20, 1857.

Tyler, Edward R., New Haven, Conn. (1). Born Aug. 3, 1800. Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).

Vanuxem, Lardner, Bristol, Pa. (1). Born in Philadelphia, Pa., July 23, 1792. Died in Bristol, Pa., June 25, 1848.

Vaux, William Sanson, Philadelphia, Pa. (1). Born in Philadelphia, May 19, 1811. Died in Philadelphia, May 5, 1882.

Wadsworth, James Samuel, Genesee, N. Y. (2). Born in Geneseo, N. Y., October 30, 1807. Died near Chancellorville, Va., May 8, 1864.

Wagner, Tobias, Philadelphia, Pa. (9).

Walker, J. R., Bay Saint Louis, Miss. (19). Born Aug. 7, 1830. Died June 22, 1887.

Walker, Joseph, Oxford, N. Y. (10).

Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.

Walker, Timothy, Cincinnati, Ohio (4). Born in Wilmington, Mass., Dec. 1, 1802. Died in Cincinnati, Ohio, Jan. 15, 1856.

Walling, H. F., Cambridge, Mass. (16). Died April 8, 1888.

Walsh, Benjamin D., Rock Island, Ill. (17).

Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.

Warnecke, Carl, Montreal, Can. (31). Died May 14, 1886.

Warren, Geo. Washington, Boston, Mass. (18). Died in 1884.

Warren, Gouverneur Kemble, Newport, R. I. (12). Born in Cold Spring N. Y., Jan. 8, 1830. Died in Newport, R. I., Aug. 8, 1882.

Warren, John Collins, Boston, Mass. (1). Born in Boston, Mass., Aug. 1, 1778. Died in Boston, May 4, 1856.

Warren, Samuel D., Boston, Mass. (29). Born in 1817. Died May 11, 1888.

Watertown, Charles, Wakefield, Eng. (1). Born in Wakefield, England. Died in Wakefield, May 26, 1865.

Watkins, Samuel, Nashville, Tenn. (26).
Watson, James Craig, Ann Arbor, Mich. (18). Born in Fingal, Canada, Jan. 28, 1838. Died in Madison, Wis., Nov. 23, 1880.
Webster, Horace B., Albany, N. Y. (1). Born in 1812. Died Dec. 8, 1843.
Webster, J. W., Cambridge, Mass. (1). Born in 1793. Died Aug. 30, 1850.
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.
Welsh, John, Philadelphia, Pa. (33). Died May, 1886.
Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.
Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.
Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.
Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.
Whitall, Henry, Camden, N. J. (33).
White, Samuel S., Philadelphia, Pa. (28). Died Dec. 30, 1879.
Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.
Whitman, Edmund B., Cambridge, Mass. (29). Died Sept. 2, 1883.
Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.
Whittlesey, Charles, Cleveland, Ohio (1). Born in Southington, Conn., Oct. 5, 1808. Died Oct. 18, 1886.
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Wilder, Graham, Louisville, Ky. (30). Born July 1, 1843. Died Jan. 16, 1885.
Willard, Emma C. Hart, Troy, N. Y. (15). Born in Berlin, Conn., Feb. 23, 1787. Died in Troy, N. Y., April 15, 1870.
Williams, Frank, Buffalo, N. Y. (25). Died Aug. 13, 1884.
Williams, P. O., Watertown, N. Y. (24).
Williamson, Robert S., San Francisco, Cal. (12). Born in New York about 1825.
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Born in Shelbyville, Ky., Feb. 6, 1826. Died in Cambridge, Mass., June 11, 1875.
Woodbury, Levi, Portsmouth, N. H. (1). Born in Francistown, N. H., Dec. 22, 1789. Died Sept. 4, 1851.
Woodman, John Smith, Hanover, N. H. (11). Born in Durham, N. H., Sept. 6, 1819. Died in Durham, N. H., May 15, 1871.
Woodward, Joseph Janvier, Washington, D. C. (28). Born in Philadelphia, Pa., Oct. 30, 1833. Died near that city, Aug. 17, 1884.
Worthen, Amos Henry, Springfield, Ill. (5). Born Oct. 31, 1813. Died May 6, 1888.

Wright, Elizur, Boston, Mass. (31). Born in South Canaan, Conn., Feb. 12, 1804. Died Nov. 20, 1885.

Wright, Harrison, Wilkes Barre, Pa. (29). Born July 15, 1850. Died Feb. 20, 1885.

Wright, John, Troy, N. Y. (1).

Wyman, Jeffries, Cambridge, Mass. (1). Born in Chelmsford, Mass., Aug. 11, 1814. Died in Bethelem, N. H., Sept. 4, 1874.

Wyckoff, William Cornelius, New York, N. Y. (20). Born in New York, N. Y., May 28, 1832. Died in Brooklyn, N. Y., May 2, 1888.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.

Youmans, Edward Livingston, New York, N. Y. (6). Born in Coeymans, N. Y., June 3, 1821. Died Jan. 18, 1887.

Young, Ira, Hanover, N. H. (1). Born in Lebanon, N. H., May 23, 1801. Died in Hanover, N. H., Sept. 14, 1858.

ADDRESS

BY

S. P. LANGLEY,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

THE HISTORY OF A DOCTRINE.

"Man, being the servant and interpreter of nature, can do and understand so much, and so much only, as he has observed, in fact or in thought, of the course of nature. Beyond this he neither knows anything nor can do anything."—BACON's *Novum Organum*, aphorism I.

IN these days, when a man can take but a very little portion of knowledge to be his province, it has become customary that your president's address shall deal with some limited topic, with which his own labors have made him familiar; and accordingly I have selected as my theme, the history of our present views about radiant energy, not only because of the intrinsic importance of the subject, but because the study of this energy in the form of radiant heat is one to which I have given special attention.

Just as the observing youth, who leaves his own household to look abroad for himself, comes back with the report that the world, after all, is very like his own family, so may the specialist, when he looks out from his own department, be surprised to find that, after all, the history of the narrowest specialty is strangely like that of scientific doctrine in general, and contains the same lessons for us. To find some of the most useful ones, it is important, however, to look with our own eyes at the very words of the masters themselves, and to take down the dusty copy of Newton, or Boyle, or Leslie, instead of a modern abstract; for, strange as it may seem, there is something of great moment in the original that has never yet been incorporated into any encyclopædia, something really es-

sential in the words of the man himself which has not been indexed in any text-book, and never will be.

It is not for us, then, here to-day, to try

> "How index-learning turns no student pale,
> Yet holds the eel of science by the tail;"

but, on the contrary, to remark that from this index-learning, from these histories of science and summaries of its progress, we are apt to get wrong ideas of the very conditions on which this progress depends. We often hear it, for instance, likened to the march of an army toward some definite end; but this, it has seemed to me, is not the way science usually does move, but only the way it seems to move in the retrospective view of the compiler, who probably knows almost nothing of the real confusion, diversity, and retrograde motion of the individuals comprising the body, and only shows us such parts of it as he, looking backward from his present standpoint, now sees to have been in the right direction.

I believe this comparison of the progress of science to that of an army, which obeys an impulse from one head, has more error than truth in it; and, though all similes are more or less misleading, I would prefer to ask you to think rather of a moving crowd, where the direction of the whole comes somehow from the independent impulses of its individual members; not wholly unlike a pack of hounds, which, in the long-run, perhaps catches its game, but where, nevertheless, when at fault, each individual goes his own way, by scent, not by sight, some running back and some forward; where the louder-voiced bring many to follow them, nearly as often in a wrong path as in a right one; where the entire pack even has been known to move off bodily on a false scent;—for this, if a less dignified illustration, would be one which had the merit of having a truth in it, left out of sight by the writers of text-books.

At any rate, the actual movement has been tortuous, or often even retrograde, to a degree of which you will get no idea from the account in the text-book or encyclopædia, where, in the main, only the resultant of all these vacillating motions is given. With rare exceptions, the backward steps—that is, the errors and mistakes, which count in reality for nearly half, and sometimes for more than half, the whole—are left out of scientific history; and the reader, while he knows that mistakes have been made, has no just idea how intimately error and truth are mingled in a sort of chemical union, even in the work of the great discoverers, and how

it is the test of time chiefly,—which enables us to say which *is* progress, when the man himself could not. If this be a truism, it is one which is often forgotten, and which we shall do well to here keep before us.

This is not the occasion to review the vague speculations of the ancient natural philosophers from Aristotle to Zeno, or to give the opinion of the schoolmen on our subject. We take it up with the immediate predecessors of Newton, among whom we may have been prepared to expect some obscure recognition of heat as a mode of motion, but where it has been, to me at least, surprising, on consulting their original works, to find how general and how clear an anticipation of our modern doctrine may be fairly said to exist. Whether this early recognition be a legacy from the Lucretian philosophy, it is not necessary to here consider. The interesting fact, however it came about, is the extent to which seventeenth-century thought is found to be occupied with views which we are apt to think very recent.

Descartes, in 1664, commences his " Le Monde " by a treatise on the propagation of light, and what we should now call radiant heat by vibrations, and further associates this view of heat as motion with the distinct additional conception, that in the cause of light and radiant heat we may expect to find something quite different from the sense of vision or of warmth; and he expresses himself with the aid of the same simile of sound employed by Draper over two hundred years later. The writings of Boyle on the mechanical production of heat contain illustrations (like that of the hammer driving the nail, which grows hot in proportion as its bodily motion is arrested) which show a singularly complete apprehension of views we are apt to think we have made our own; and it seems to me that any one who consults the originals will admit, that, though its full consequences have not been wrought out till our own time, yet the fundamental idea of heat as a mode of motion is so far from being a modern one, that it was announced in varying forms by Newton's immediate predecessors, by Descartes, by Bacon, by Hobbes, and in particular by Boyle, while Hooke and Huyghens merely continue their work, as at first does Newton himself.

If, however, Newton found the doctrine of vibrations already, so to speak, " in the air," we must, while recognizing that in the

history of thought the new always has its root in the old, and that it is not given even to a Newton to create an absolutely new light, still admit that the full dawn of our subject properly begins with him, and admit, too, that it is a bright one, when we read in the "Optics" such passages as these:—

"Do not all fixed bodies, when heated beyond a certain degree, emit light and shine, and is not this emission performed by the vibrating motions of their parts?" And again: Do not several sorts of rays make vibrations of several bignesses?" And still again: "Is not the heat conveyed by the vibrations of a much subtler medium than air?"

Here is the undulatory theory; here is the connection of the ethereal vibrations with those of the material solid; here is "heat as a mode of motion;" here is the identity of radiant heat and light; here is the idea of wave-lengths. What a step forward this first one is! And the second?

The second is, as we know, backward. The second is the rejection of this, and the adoption of the corpuscular hypothesis, with which alone the name of Newton (a father of the undulatory theory) is, in the minds of most, associated to-day.

Do not let us forget, however, that it was on the balancing of arguments from the facts then known, that he decided, and that perhaps it was rather an evidence of his superiority to Huyghens, that apprehending equally clearly with the latter, the undulatory theory, he recognized also more clearly that this theory, as then understood, utterly failed to account for several of the most important phenomena. With an equally judicial mind, Huyghens would perhaps have decided so too, in the face of difficulties, all of which have not been cleared up even to-day. These two great men, then, each looked around in the then darkness as far as his light carried him. All beyond that was chance to each; and fate willed that Newton, whose light shone farther than his rival's, found it extend just far enough to show the entrance to the wrong way. He reaches the conclusion that we all know; one not only wrong in regard to light, but which bears pernicious results on the whole theory of heat, since light being conceded to be material, radiant heat, if affiliated to light, must be regarded as material too, and Newton's influence is so permanent, that we shall see this strange conclusion drawn by the contemporaries of Herschel from his experiments made a hundred years later.

It would seem then that the result of this unhappy corpuscular theory was more far-reaching than we commonly suppose, and that it is hardly too much to say that the whole promising movement of that age toward the true doctrine of radiant energy is not only arrested by it, but turned the other way; so that in this respect the philosophy of fifty years later is actually farther from the truth than that of Newton's predecessors, and the immense repute of Newton as a leader, on the whole so rightly earned, here leads astray others than his conscious disciples, and, it seems to me, affects men's opinions on topics which appear at first far removed from those he discussed. The adoption of phlogiston was, as we may reasonably infer, facilitated by it, and remotely Newton is perhaps also responsible in part for the doctrine of caloric a hundred years later. After him, at any rate, there is a great backward movement. We have a distinct retrogression from the ideas of Bacon and Hobbes and Boyle. Night settles in again on our subject almost as thick as in the days of the schoolmen, and there seems to be hardly an important contribution to our knowledge, in the first part of the eighteenth century, due to a physicist.

"Physics, beware of metaphysics," said Newton,—words which physicists are apt so exclusively to quote, that it seems only due to candor to observe that the most important step, perhaps, in the fifty years which followed the "Optics," came from Berkeley, who, reasoning as a metaphysician, gave us during Newton's lifetime a conception wonderfully in advance of his age. Yet the "New Theory of Vision" was generally viewed by contemporary philosophers as only an amusing paradox, while "coxcombs vanquish[ed] Berkeley with a grin;" and this contribution to science,—an exceptional if not a unique instance of a great physical generalization reached by *a priori* reasoning,—though published in 1709, remains in advance of the popular knowledge even in these closing years of the nineteenth century.

In the meantime a new error had risen among men,—a new truth, as it seemed to them,—and a thing destined to have a strong reflex action on the doctrine of radiant energy. It began with the generalization of a large class of phenomena which we now associate with the action of oxygen, then of course unknown, a generalization useful in itself, and accompanied by an explanation which was not in its origin objectionable. Let us consider, in illustration,

any familiar instance of oxidation, and try to look first for what was reasonable in the eighteenth-century views of the cause of such phenomena.

A piece of dry wood has in it the power of giving out heat and light when set on fire; but after it is consumed there is left of it only inert ashes, which can give neither. Something, then, has left the wood in process of becoming ashes; virtue has gone out of it, or, as we should say, its potential energy has gone.

This is, so far, an important observation, extending over a wide range of phenomena, and, if it had presented itself to the predecessors of Newton, it would probably have been allied to the vibratory theories, and become proportionately fruitful. But to his disciples, and to chemists and others, who, without being perhaps disciples, were like all then, more or less consciously influenced by the materiality of the corpuscular theory, it appeared that this virtue also was a material emanation;— that this energy was an actual ingredient of the wood,— a crudeness of conception which seems most strange to us, but is not perhaps unaccountable in view of the then current thought.

I have said that the progress of science is not so much that of an army as of a crowd of searchers, and that a call in a false direction may be responded to, not by one only, but by the whole body. In illustration, observe that during the greater part of the entire eighteenth century, this doctrine was adopted by almost every chemist and by many physicists. It had as general an acceptance among chemists then as the kinetic theory of gases, for instance, has among physicists now, and, so far as time is any test of truth, it was tested more severely than the kinetic theory has yet been; for it was not only the lamp and guide of chemists, and to a great extent of physicists also, but it remained the time-honored and highest generalization of chemical science for over half a century, and it was accepted not so much as a conditional hypothesis, as a final guide, and a conquest for truth which should endure always. And now where is it? Dissipated so utterly from men's minds, that, to the unprofessional part of even an educated audience like this, "phlogiston," once a name to conjure with, has become an unmeaning sound.

There is no need to insist on the application of the obvious moral to hypotheses of our own day.

I have tried to recall for a moment all that "phlogiston" meant

a little more than a hundred years ago, partly because it seems to me, that, though a chemical conception, physics is not blameless for it, but chiefly because before it quitted the world it appears to have returned to physics the wrong in a multiplied form, by generating an offspring specially inimical to true ideas about radiant heat, and which is represented by a yet familiar term. I mean "caloric."

This word is still used loosely as a synonyme for heat, but has quite ceased to be the very definite and technical term it once was. To me it has been new to find that this so familiar word "caloric," so far as my limited search has gone, was apparently coined only toward the last quarter of the last century. It is not to be found in the earliest edition of Johnston's dictionary, and, as far as I can learn, appears first in the corresponding French form in the works of Fourcroy. It expressed an idea which was the natural sequence of the phlogiston theory, and which is another illustration that the evil which such theories do lives after them.

"Caloric" first seemingly appears, then, as a word coined by the French chemists, and meant originally to signify the unknown cause of the sensation heat, without any implication as to its nature. But words, we know, though but wise men's counters, are the money of fools; and this one very soon came to commit its users to an idea which was more likely to have had its origin in the mind of a chemist at that time than of any other,—the idea of the cause of heat as a material ingredient of the hot body; something not, it is true, having weight, but which it would have been only a slight extension of the conception, to think might one day be isolated by a higher chemical art, and exhibited in a tangible form.

We may desire to recognize the perverted truth which usually underlies error and gives it currency, and be willing to believe that even "caloric" may have had some justification for its existence; but this error certainly seems to have been almost altogether pernicious for nearly the next eighty years, and down even to our own time. With this conception as a guide to the philosophers of the last years of the eighteenth century, it is not, at any rate, surprising if we find that at the end of a hundred years from Newton the crowd seems to be still going constantly farther and farther away from its true goal.

The doctrine of caloric is, however, always recognized as an hypothesis more acceptable to chemists than to physicists, some of

whom still stand out for the theories of Newton's predecessors, even through the darkest years; so that the old idea of heat, as a mode of motion, has by no means so utterly died that it does not appear here and there during the last century, and indeed, not only among philosophers, but even in a popular form.

In an old English translation of Father Regnault's compilation on physics, dated about 1730, I find, for instance, the most explicit statement of the doctrine of heat as a mode of motion. Here heat is defined (with the aid of a simile due, I believe, to Boyle) as "any Agitation whatever of the insensible parts. Thus a Nail which is drove into the Wood by the stroke of a Hammer does not appear to be hot, because its immediate parts have but one common Movement. But should the Nail cease to drive, it would acquire a sensible Heat, because its insensible Parts which receive the Motion of the Hammer now acquire an agitation every way rapid." We certainly must admit that the user of this illustration had just and clear ideas; and the interesting point here appears to be, that as Father Regnault's was not an original work, but a mere compendium or popular scientific treatise of the period, we see, if only from this instance, that the doctrine of heat as a mode of motion was not confined to the great men of an earlier or a later time, but formed a part of the common pabulum during the eighteenth century to an extent that has been forgotten.

Although Prevost gave us his most material contribution about 1790, we have, it seems to me, on the whole, little to interest us during that barren time in the history of radiant energy called the eighteenth century,— a century in which science wore the pedant's cap and gown, and her students read the poem of Creation like grammarians, for its syntax;— a century whose latter years are given up, till near its very close, to bad *a priori* theories in our subject, except in the work of two Americans; for in the general dearth at this time of experiments in radiant heat, it is a pleasure to fancy Benjamin Franklin sitting down before the fire, with a white stocking on one leg and a black one on the other, to see which leg would burn first, and to recall again how Benjamin Thompson (Count Rumford) not only weighed "caloric" literally in the balance and found it wanting, but made that memorable experiment in the Munich founderies which showed that heat was perpetually and without limit created from motion.

It was in the last years of the century, too, that he provided for

the medal called by his name, and which, though to be given for researches in heat and light, has, I believe, been allotted in nearly every instance to men, who, like Leslie, Malus, Davy, Brewster, Fresnel, Melloni, Faraday, Arago, Stokes, Maxwell, and Tyndall, have contributed toward the subject of radiant energy in particular.

We observe that before this time the scientific literature of the century scarcely considers the idea even of radiant heat, still less of radiant energy, so that we have been obliged here to discuss the views of its physicists about heat in general, heat and light in most minds being then distinct entities; all the ways for pilgrims to this special shrine of truth being barred, like those in Bunyan's Pilgrim's Progress, by the two unfriendly giants who are here called Phlogiston and Caloric, so that there are few scientific pilgrims who do not pay them toll.

The last years of this century were destined to see the most remarkable experiments in heat made in the whole of the hundred; for the memoir of Rumford appeared in the Philosophical Transactions for 1798; and in the very year 1800 appeared in the same place Sir William Herschel's paper, in which he describes how he placed a thermometer in successive colors of the solar spectrum, finding the heat increase progressively from the violet to the red, and increase yet more beyond the red where there was no color or light whatever; so that there are, he observes, invisible rays as well as visible. More than that, the first outnumber the second; and these dark rays are found in the very source and fount of light itself. These dark rays can also be obtained, he observes, from a candle or a piece of non-luminous hot iron, and, what is very significant, they are found to pass through glass, and to be refracted by it like luminous ones.

And now Herschel, searching for the final verity through a series of excellent experiments, asks a question which shows that he has truth, so to speak, in his hands,— he asks himself the great question whether heat and light be occasioned by the same or different rays.

Remember the importance of this (which the querist himself fully recognized); remember, that, after long hunting in the blindfold search, he has laid hands, as we now know, on the truth herself, and then see him — let go. He decides that heat and light are not occasioned by the same rays, and we seem to see the fugi-

tive escape from his grasp, not to be again fairly caught till the next generation.

I hardly know more remarkable papers than these of Herschel's in the Philosophical Transactions for 1800, or anything more instructive in little men's successes than in this great man's failure, which came in the moment of success. I would strongly recommend the reading of these remarkable original memoirs to any physicist who knows them only at second-hand.

One more significant lesson remains, in the effect of this on the minds of his contemporaries. Herschel's observation is to us almost a demonstration of the identity of radiant heat and light; but now, though the nineteenth century is opening, it is with the doctrine in the minds of most physicists, and perhaps of all chemists, that heat is occasioned by a certain material fluid. Phlogiston is by this time dead or dying, but Caloric is very much alive, and never more perniciously active than now, when, for instance, years after Herschel's observation, we find this cited as "demonstrating the existence of caloric;"— which was, it seems, the way it looked to a contemporary.

In the year 1804 appeared what should be a very notable book in the history of our subject, written by Sir John Leslie, whose name survives perhaps in the minds of many students chiefly in connection with the "cube" which is still called after him.

Leslie, however, ought to be remembered as a man of original genius, worthy to be mentioned with Herschel and Melloni; and his, too, is one of the books which the student may be recommended to read, at least in part, in the original; not so much for the writer's instructive experiments (which will be found in our text-books) as for his most instructive mistakes, which the text-book will probably not mention.

He began by introducing the use of the simple instrument which bears his name, and a new and more delicate heat-measurer (the differential thermometer); and with these, and concave reflectors of glass and metal, he commenced experiments in radiant heat, than which, he tells us, no part of physical science then appeared so dark, so dubious, and so neglected. It is interesting, and it marks the degree of neglect he alludes to, that his first discovery was that different substances have different radiating and absorbing powers. It gives us a vivid idea of the density of previous ignorance, that it was left to the present century to demonstrate

this elementary fact, and that Leslie, in view of such discoveries, says, "I was transported at the prospect of a new world emerging to view."

Next he shows that the radiating and absorbing powers are proportional, next that cold as well as heat seems to be radiated, and next undertakes to see whether this radiant heat has any affinity to light. He then experiments in the ability of radiant heat to pass through a transparent glass, which transmits light freely, and thinks he finds that none does pass. Radiant heat with him seems to mean heat from non-luminous sources; and the ability or non-ability of this to pass through glass, is to Leslie and his successors a most crucial test, and its failure to do so a proof that this heat is not affiliated to light.

Let us pause a moment here to reflect that we are apt to unconsciously assume, while judging from our own present standpoint where past error is so plain, that the false conclusion can only be chosen by an able, earnest, conscientious seeker, after a sort of struggle. Not so. Such a man is found welcoming the false with rapture, as very truth herself.

"What, then," says Leslie, "is this calorific and frigorific fluid after which we are inquiring? It is not light, it has no relation to ether, it bears no analogy to the fluids, real or imaginary, of magnetism and electricity. But why have recourse to invisible agents? *Quod petis, hic est.* It is merely the ambient AIR."

The capitals are Leslie's own, but ere we smile with superior knowledge, let us put ourselves in his place, and then we may comprehend the exultation with which he announces the identity of radiant heat and common air, for he feels that he is beginning a daring revolt against the orthodox doctrine of caloric; and so he is.

The first five years of this century are notable in the history of radiant energy, not only for the work of Leslie, and for the observation by Wollaston, Ritter, and others, of the so-called "chemical" rays beyond the violet, but for the appearance of Young's papers, reëstablishing the undulatory theory, which he indeed considered in regard to light, but which was obviously destined to affect most powerfully the theory of radiant energy in general.

We are now in the year 1804, or over a century and a quarter since the corpuscular theory was emitted, and during that time it has gradually grown to be an article of faith in a sort of scientific church, where Newton has come to be looked on as an infallible

head, and his views as dogmas, about which no doubt is to be tolerated; but if we could go back to Cambridge in the year 1668, when the obscure young student, in no way conscious of his future pontificate, takes his degree (standing twenty-third on the list of graduates), we should probably find that he had already elaborated and greatly improved certain already current ideas into the undulatory theory of light, which he at any rate promulgated a few years later, and afterward, pressed with many difficulties, altered, as we now know, to an emissive one.

Probably, if we could have heard his own statement then, he would have told how sorely tried he was between these two opinions, and, while explaining to us how the wavering balance came to lean as it did, would have admitted, with the modesty proper to such a man, that there was a great deal to be said on either side. We may, at any rate, be sure that it would not be from the lips of Newton himself that we should have had this announced as a belief which was to be part of the rule of faith to any man of science.

But observe how, if science and theology look askance at each other, it is still true that some scientific men and some theologians have, at any rate, more in common than either is ready to admit; for at the beginning of this century Newton's followers, far less tolerant than their master, have made out of this modest man a scientific pontiff, and out of his diffident opinions a positive dogma, till as years go on, he comes to be cited as so infallible that a questioning of these opinions is an offence deserving excommunication.

This has grown to be the state of things in 1804, when Young, a man possessing something of Newton's own greatness, ventures to put forward some considerations to show that the undulatory theory may be the true one, after all. But the prevalent and orthodox scientific faith was still that of the material nature of light; the undulatory hypothesis was a heresy, and Young a heretic. If his great researches had been reviewed by a physicist or a brother worker, who had himself trodden the difficult path of discovery, he might have been treated at least intelligently; but then, as always, the camp-followers, who had never been at the front, shouted from a safe position in the rear to the man in the dust of the fight, that he was not proceeding according to the approved rules of tactics; then, as always, these men stood between the public and the investigator, and distributed praise or blame.

If you wish to hear how the scientific heretic should be rebuked

for his folly, listen to one who never made an observation, but, having a smattering of everything books could teach about every branch of knowledge, was judged by himself and by the public to be the fittest interpreter to it, of the physical science of this day. I mean Henry Brougham, the universal critic, the future Lord-chancellor of England, of whom it was observed, that, "if he had but known a little *law*, he would have known a little of everything." He uses the then all-powerful *Edinburgh Review* for his pulpit, and from it fulminates the condemnations of the church on the innovating memoir of the heretical Young.

"This paper," he says, "contains nothing which deserves the name of experiment or discovery; and it is, in fact, destitute of every species of merit . . . first is another lecture, containing more fancies, more blunders, more unfounded hypotheses, more gratuitous fictions . . . and all from the fertile yet fruitless brain of the eternal Dr. Young. In our second number we exposed the absurdity of this writer's 'law of interference,' as it pleases him to call one of the most incomprehensible suppositions that we remember to have met with in the history of human hypotheses."

There are whole pages of it, but this is enough; and I cite this passage among many such at command, not only as an example of the way the undulatory theory was treated at the beginning of this century in the first critical journal of Europe, but as another example of the general rule that the same thing may appear intrinsically absurd, or intrinsically reasonable, according to the year of grace in which we hear of it. The great majority, even of students of science, must take their opinions ready-made as to science in general; each knowing, so far as he can be said to know anything at first-hand, only that little corner which research has made specially his own.

The moral we can all draw, I think, for ourselves.

In spite of such criticism as this, the undulatory hypothesis of light made rapid way, and carried with it, one would now say, the necessary inference that radiant heat was due to undulations also. This was, however, no legitimate inference to those to whom radiant heat was still a fluid; and yet, in spite of all, the modern doctrine now begins to make visible progress.

A marked step is taken about 1811 by a young Frenchman, De la Roche, who deserves to be better remembered than he is, for he clearly anticipated some of Melloni's discoveries. De la Roche in

particular shows that of two successive screens the second absorbs heat in a less ratio than the first; whence he, before any one else, I believe, derives the just and most important, as well as the then most novel conception, that radiant heat is of different *kinds*. He sees also, that, as a body is heated more and more, there is a gradual and continual advance not only in the amount of heat it sends out, but in the kind, so that, as the temperature still rises, the radiant heat becomes light by imperceptible gradations; and he concludes that heat and light are due to one simple agent, which, as the temperature rises yet more, appears more and more as light, or which, as the luminous radiation is absorbed, re-appears as heat. Very little of it, he observes, passes even transparent screens at low temperatures, but more and more does so as the temperature rises.

All this is a truism in 1888, but it appears admirably new as well as true in 1811; and if De la Roche had not been removed by an early death, his would have not improbably been the greatest name of the century in the history of our subject; an honor, however, which was in fact reserved for another.

The idea of the identity of light and radiant heat had by this time made such progress that the attempt to polarize the latter was made in 1818 by Berard. We have just seen in Herschel's case how the most sound experiment may lead to a wrong conclusion, if it controvert the popular view. We now have the converse of this in the fact that the zeal of those who are really in the right way may lead to unsound and inconclusive experiment; for Berard experimentally established, as it was supposed, the fact that obscure radiant heat can be polarized. So it can, but not with such means as Berard possessed, and it was not till a dozen years more that Forbes actually proved it.

At this time, however fairly we seem embarked on the paths of study which are followed to-day, and while the movement of the main body of workers is in the right direction, it is yet instructive to observe how eminent men are still spending great and conscientious labor, their object in which is to advance the cause, while the effect of it is to undo the little which has been rightly done, and to mislead those who have begun to go right.

As an instance both of this and of the superiority of modern apparatus, we may remark,—after having noticed that the ability of obscure heat to pass through glass, if completely established, would

be a strong argument in favor of its kinship to light, and that De la Roche and others had indicated that it would do so (in which we now know they were right),—that at this stage, or about 1816, Sir David Brewster, the eminent physicist, made a series of experiments which showed that it would not so pass. Ten years later, in view of the importance of the theoretical conclusion, Baden Powell repeated his observations with great care, and confirmed them, announcing that the earlier experimenters were wrong, and that Brewster was right; so that here all these years of conscientious work resulted in establishing, so far as it could be established, a wholly wrong conclusion in place of a right one already gained.

It may be added, that with our present apparatus, the passage of obscure radiant heat through glass could be made convincingly evident in an experiment which need not last a single second.

We are now arrived at a time when the modern era begins; and in looking back over one hundred and fifty years, from the point of view of the experimenter himself, with his own statement of the truth as he saw it, we find that the comparison of the progress of science to that of an army, which moves, perhaps with the loss of occasional men, but on the whole victoriously and in one direction, is singularly misleading; and I state this more confidently here, because there are many in this audience who did not get their knowledge of nature from books only, but who have searched for the truth themselves; and, speaking to them, may I not say that those who have so searched know that the most honest purpose and the most patient striving have not been guaranties against mistakes,—mistakes which were probably hailed at the time as successes? It was some one of the fraternity of seekers, I am sure, who said, "Show me the investigator who has never made a mistake, and I will show you one who has never made a discovery."

We have seen the whole scientific body, as regards this particular science of radiant energy, moving in a mass, in a wrong direction, for a century; we have seen that individuals in it go on their independent paths of error; and we can only wonder that an era should have come in which such a real advance is made as in ours.

That era has been brought in by the works of many, but more than by any other through the fact that, in the year 1801, there came into the world at Parma an infant who was born a physicist,

as another is born a poet; nay, more; who was born, one might say, a devotee of one department of physics, that of radiant heat; being affected in his tenderest years with such a kind of precocious passion for the subject as the childish Mozart showed for music. He was ready to sacrifice everything for it; he struggled through untold difficulties, not for the sake of glory or worldly profit, but for radiant heat's sake; and when fame finally came to him, and he had the right to speak of himself, he wrote a preface to his collected researches, which is as remarkable as anything in his works. In this preface he has given us, not a summary of previous memoirs on the subject, not a table of useful factors and formulæ, not anything at all that an English or an American scientific treatise usually begins with, but the ingenuous story of his first love, of his boyish passion for this beloved mistress; and all this with a trust in us his readers which is beautiful in its childlike confidence in our sympathy.

I should need to abbreviate and injure in order to quote; but did ever a learned physical treatise and collection of useful tables begin like this before?

"I was born at Parma, and when I got a holiday used to go into the country the night before, and go to bed early, so as to get up before the dawn. Then I used to steal silently out of the house, and run, with bounding heart, till I got to the top of a little hill, where I used to set myself so as to look toward the East." There, he tells us, he used, in the stillness of nature, to wait the rising sun, and feel his attention rapt, less with the glorious spectacle of the morning light itself than with the sense of the mysterious heat which accompanied its beams, and brought something more necessary to our life and that of all nature than the light itself, so that the idea that not only mankind, but nature, would perish though the light continued, if this was divorced from heat, made a profound impression, he tells us, on his childish mind.

The statement that such an idea could enter with dominating force into the mind of a child will perhaps seem improbable to most. It will, however, be credible enough to some here, I have no doubt.

Is there some ornithologist present who remembers a quite infantile attraction which birds possessed for him above all the rest of the animated creation? some chemist whose earliest recollections are of the strange and quite abnormal interest he found as a child in making experimental mixtures of every kind of accessible house-

hold fluid and solid? some astronomer who remembers that when a very little creature not only the sight of the stars, but of any work on astronomy, even if utterly beyond his childish comprehension, had an incomprehensible attraction for him? I will not add to the list. There are, at any rate, many here who will understand Melloni when he tells how this radiant heat, commonplace to others, was wonderful to his childish thought, and wrought a charm on it such that he could not see wood burn in a fireplace, or look at a hot stove, without its drawing his mind, not to the fire or iron itself, but to the mysterious effluence which it sent.

This was the youth of genius; but let not any fancy that genius in research is to be argued from such premonitions alone, unless it can add to them that other qualification of genius which has caused it to be named the faculty of taking infinite pains. Melloni's subsequent labors justified this last definition also; but I cannot speak of them here, further than to say, that, after going over a large part of his work myself, with modern methods and with better apparatus, he seems to me the man, of all great students of our subject, who, in reference to what he accomplished, made the fewest mistakes.

Melloni is very great as an experimenter, and owes much of his success to the use of the newly invented thermopile, which is partly his own. I can here, however, speak only of his results, and of but two of these,— one generally known; the other, and the more important, singularly little known, at least in connection with him.

The first is the full recognition of the fact, partly anticipated by De la Roche, that radiant heat is of different kinds, and that the invisible emanations differ among themselves just as those of light do. Melloni not only established the fact, but invented a felicitous term for it, which did a great deal to stamp it on recognition,— the term "thermochrose," or heat-color, which helps us to remember, that, as the visible and apparently simple emanation of light is found to have its colors, so radiant heat, the invisible but apparently simple emanation, has what would be colors to an eye that could see them. This result is well known in connection with Melloni.

The other and the greater, which is not generally known as Melloni's, is the generalization that heat and light are effects of one and the same thing, and merely different manifestations of it.

I translate this important statement as closely as possible from his own words. They are that

"*Light is merely a series of calorific indications sensible to the organs of sight, or vice versa, the radiations of obscure heat are veritable* INVISIBLE RADIATIONS *of light.*"

The italics and the capitals are Melloni's own. He wishes to have no ambiguity about his announcement behind which he may take shelter; and he had so firm a grasp of the great principle, that, when his first attempts to observe the heat of the moon failed, he persevered, because this principle assured him that where there was light there must be heat. This statement was made in 1843, and ought, I think, to insure to Melloni the honor of being first to thus distinctly announce this great generalization.

The announcement passed apparently unnoticed, in spite of his acknowledged authority; and the general belief not merely in different entities in the spectrum, but in a material caloric, continued as strong as ever. If you want to see what a hold on life error has, and how hard it dies, turn to the article "heat," in the eighth edition of the "Encyclopædia Britannica," where you will find the old doctrine of caloric still in possession of the field in 1853; and still later, in the generally excellent "English Encyclopædia" (edition of 1867), the doctrine of caloric is, on the whole, preferred to the undulatory hypothesis. It is very probable that a searcher might find many traces of it yet lingering among us; so that Giant Caloric is not, perhaps, even yet quite dead, though certainly grown so crazy and stiff in the joints, that he can now harm pilgrims no more.

So far as I know, no physicist of eminence reässerted Melloni's principle with equal emphasis, till J. W. Draper, in 1872. Only sixteen years ago, or in 1872, it was almost universally believed that there were three different entities in the spectrum, represented by actinic, luminous and thermal rays. Draper remarks that a ray consists solely of ethereal vibrations whose lost *vis viva* may produce either heat or chemical change. He uses Descartes' analogy of the vibration of the air, and sound; but he makes no mention either of Descartes or of Melloni, and speaks of the principle as leading to a modification of views then "universally" held. Since that time the theory has made such rapid progress, that, though some of the older men in England and on the European continent

have not welcomed it, its adoption among all physicists of note may be said to be now universal, and a new era in our history begins with it. I mean with the recognition that there is one radiant energy which appears to us as "actinic," or "luminous" or "thermal" radiation, according to the way we observe it. Heat and light, then, are not things in themselves, but whether different sensations in our own bodies, or different effects in other bodies, are merely effects of this mysterious thing we call radiant energy, without doing more in this than give a name to the ignorance which still hangs over the ultimate cause.

I am coming down dangerously near our own time, for one who would be impartial in dealing with names of those still living. In such a brief review of this century's study of radiant energy in other forms than light, it has been necessary to pass without mention the labors of such men as Pouillet and Becquerel in France, of Tyndall in England, and of Henry in America. It has been necessary to omit all mention of those who have advanced the knowledge of radiant energy as light, or I should have had to speak of labors so diverse as those of Fraunhofer, of Kirchoff, of Fresnel, of Stokes, of Lockyer, of Jannsen, and many more. I have made no mention, in the instructive history of error, of many celebrated experimental researches; in particular of such a problem as the measurement of solar heat, great in importance, but apparently most simple in solution, yet which has now been carried on from generation to generation, each experimenter materially altering the result of his predecessor, and where our successors will probably correct our own results in turn. I have not spoken of certain purely experimental investigations, like those of Dulong and Petit, which have involved immense and conscientious labor, and have apparently rightly earned the name of "classic" from one generation, only to be recognized by the next as leading to untrustworthy results, and leaving the work to be done again with new methods, guided by new principles.

In these instances, painstaking experiments have proved insufficient, less from want of skill in the investigator than from his ignorance of principles not established in time to enable him to interpret his experiments; but, if there were opportunity, it would be profitable to show how inexplicably sometimes error flourishes, grows, and maintains an apparently healthy appearance of truth,

without having any root whatever. Perhaps I may cite one instance of this last from my own experience.

About ten years ago it was generally believed that the earth's atmosphere acted exactly the part of the glass in a hotbed, and that it kept the planet warm by exerting a specially powerful absorption on all infra-red rays. I had been trained in the orthodox scientific church, of which I am happy to be still a member; but I had acquired perhaps an almost undue respect, not only for her doctrines, but for her least sayings. Accordingly, when my own experiments did not agree with the received statement, I concluded that my experiments must be wrong, and made them all over again, till spring, summer, autumn and winter had passed, each season giving its own testimony; and this for successive years. The final conclusion was irresistible, that the universal statement of this alleged well-known fact, inexplicable as this might seem, in so simple a matter, was directly contradicted by experiment. I had some natural curiosity to find how every one knew this to be a fact; but search only showed the same statement (that the earth's atmosphere absorbed dark heat like glass) repeated everywhere, with absolutely nowhere any observation or evidence whatever to prove it, but each writer quoting from an earlier one, till I was almost ready to believe it a dogma superior to reason, and resting on the well-known "*Quod semper, quod ubique, quod ab omnibus, creditum est.*" Finally I appear to have found its source in the writings of Fourier, who, alluding to De Saussure's experiments (which showed that dark heat passed with comparative difficulty through glass), observes that if the earth's atmosphere were solid, it would act as the glass does. Fourier simply takes this (in which he is wrong) for granted; but, as he is an authority on the theory of heat, his words are repeated without criticism, first by Poisson, then by others, and then in the text-books; and, the statement gaining weight by age, it comes to be believed absolutely, on no evidence whatever, for the next sixty years, that our atmosphere is a powerful absorber of precisely those rays which it most freely transmits.

The question of fact here, though important, is, I think, quite secondary to the query it raises as to the possible unsuspected influence of mere tradition in science, when we do not recognize it as such. Now, members of any church are doubtless consistent in believing in traditions, if they believe that these are presented to

them by an infallible guide; but are we, who have no infallible guide, quite safe in believing all we do, from our fond persuasion that in the scientific body mere tradition has no weight?

In even this brief sketch of the growth of the doctrine of radiant energy, we have perhaps seen that the history of the progress of this department of science is little else than a chapter in that larger history of human error which is still to be written, and which, it is safe to say, would include illustrations from other branches of science, as well as my own. But — and here I ask pardon if I speak of myself — I have been led to review the labors of other searchers from this standpoint, because I had first learned, out of personal experience, that the greatest care was no certain guaranty of final accuracy; that to labor in the search for a truth with such endless pains as a man might bestow if his own salvation were in question, did not necessarily bring the truth; and because, seeking to see whether this were the lot of other and greater men, I have found that it was, and that, though no one was altogether forsaken of the truth he sought, or on the whole review of his life as a seeker, but might believe he had advanced her cause, yet, when after long waiting, he saw once more what seemed her beautiful face, there was no absolute certainty that this might not be the mockery of error; and, doubtless, appeal might be made to the experience of many investigators here with the question, "Is it not so?"

What then? Shall we admit that truth is only to be surely found under the guidance of an infallible church? If there be such a church, yes! Let us, however, remember that the church of science is not such a one, and be ready to face all the consequences of the knowledge that her truths are put forward by her as provisional only, and that her most faithful children are welcome to disprove them.

What then, again? Shall we say that the knowledge of truth is not advancing? It is advancing, and never so fast as to-day; but the steps of its advance are set on past errors, and the new truths become such stepping-stones in turn.

To say that what are truths to one generation are errors to the next, or that truth and error are but different aspects of the same thing to our poor human nature, may be to utter truisms; but truisms which one has verified for one's self out of a personal experi-

ence are apt to have a special value to the owner; and these lead, at any rate, to the natural question, "Where is, then, the evidence that *we* are advancing in reality, and not in our own imagination?"

There are many here who will no doubt heartily subscribe to the belief that there is no absolute criterion of truth for the individual, and admit that there is no positive guaranty that we, with this whole generation of scientific men, may not, like our predecessors, at times go the wrong way in a body, yet who believe as certainly that science as a whole, and this branch of it in particular, is actually advancing with hitherto unknown rapidity. In asking to be included in this number, let me add that to me the criterion of this advance is not in any ratiocination, not in any *a priori* truth, still less in the dictum of any authority, but in the undoubted observation that our doctrine of radiant energy is reaching out in every direction, and proving itself by the equally undoubted fact that through its aid nature obeys us more and more; proving itself by such material evidence as is found in the electric lights in our streets, and in a thousand such ways which I need not pause to enumerate.

And here I might end, hoping that there may be some lessons for us in the history of what has been said. I will venture to ask attention to but one. It is that in these days, when the advantage of organization is so fully recognized, when there is a well-founded hope that by coöperation among scientific men knowledge may be more rapidly increased, and when not only in the great scientific departments of government but everywhere, there is a tendency to the formation of the divisions of a sort of scientific army, not to say of a scientific church — that at such a time we should yet remember, that, however rapidly science changes, human nature remains much the same; and (while we are uttering truisms) let us venture to repeat that there is a very great deal of this "human nature" even in the scientific man, whose best type is one nearly as independent as nature itself, and one which will not always work best at the word of command. Let him then never forget that the history of science, scarcely less than of theology, warns him of the tendency of authority to exceed its proper sphere, and from without it, to define belief, and to impose obedience to doctrine.

Finally, if, turning to the future, I were asked what I thought were the next great steps to be taken in the study of radiant heat,

I should feel unwilling to attempt to look more than a very little way in advance. Immediately before us, however, there is one great problem waiting solution. I mean the relation between temperature and radiation; for we know almost nothing of this, where knowledge would give new insight into almost every operation of nature (nearly every one of which is accompanied by the radiation or reception of heat), and would enable us to answer inquiries now put to physicists in vain by every department of science, from that of the naturalist as to the enigma of the brief radiation of the glow-worm, to that of the geologist who asks as to the number of million years required for the cooling of a world.

When, however, we begin to go beyond the points which seem, like this, to invite our very next steps in advance, we cannot venture to prophesy, and must content ourselves with the knowledge that through our study we are beginning to apprehend the full meaning of one of the early great ones of science, who described man as the meeting point of two infinities. That there is an infinity of space above him, man has long known, but that there is another absolute infinity, and the possibilities which lie in the infinitesimals of space, he is but beginning to realize. The secular movements, whose accomplishment demands more than a million years of time, he has already considered; but of the consequences which may result from a more careful study of actions occurring in the infinitesimals of time, and whose whole duration may be far less than the millionth of a second, he has hardly even yet begun to think; and these are but little portions of the ungarnered field of research, open to the student of that radiant energy which sustains, with our own being, that of all animated nature, of which humanity is but a part.

If there be any students of nature here, who, feeling drawn to labor in this great field of hers, still doubt whether there is yet room, surely it may be said to them, "Yes, just as much room as ever, as much room as the whole earth offered to the first man;" for everything that has been done in the past is, I believe, as nothing to what remains before us, and that field is simply unbounded. The days of hardest trial and incessant bewildering error in which your elders have wrought, seem over. You "in happier ages born," you of the younger and the coming race, who have a mind to enter in and possess it, may, as the last word here, be bidden to indulge in an equally unbounded hope.

REPORTS OF COMMITTEES.

REPORT OF THE COMMITTEE ON INDEXING CHEMICAL LITERATURE.

THE Committee on Indexing Chemical Literature respectfully presents to the Chemical Section its sixth annual report.

By the liberality of the Association the Committee secured 500 copies of the report for 1887, and these were distributed through the Secretaries of the American Chemical Society, the London Chemical Society, and the Washington Chemical Society, and directly by the Chairman of the Committee.

The Provisional List of Abbreviations of Titles of Chemical Journals, which formed Appendix B to the Report, was received by chemists with general approbation, and was reprinted in the Proceedings of the American Association for the Advancement of Science, Chemical News (London), American Chemical Journal (Baltimore), Journal of Analytical Chemistry (Easton), Journal of the American Chemical Society (New York), and was favorably noticed in the American Journal of Science (New Haven); this practically ensures its adoption.

During the year just closed, the Index to the Literature of the Spectroscope by Dr. Alfred Tuckerman has been printed by the Smithsonian Institution. This forms a work of about 400 pages, and contains 3,829 titles by 799 authors.

The Table of Specific Gravities for Solids and Liquids by Prof. F. W. Clarke is now in the compositor's hands and will soon be published by the Smithsonian Institution. This work is really a new and completely revised edition of Part I of the original "Constants of Nature."

An Index to the Literature of Columbium by Prof. Frank W. Traphagen has been completed and accepted by the Committee. Its publication has been undertaken by the Smithsonian Institution.

Dr. H. C. Bolton has compiled a Bibliography of Chemistry for 1887, the publication of which has been begun by the Smithsonian Institution.

Several chemists project indexes and have made more or less progress on them. Mr. Arthur A. Noyes is engaged on an Index to the Literature of Ethylene; Prof. William P. Mason volunteers to index Methane; Mr. William Rupp undertakes to index Cæsium and Rubidium; Professor Traphagen plans to index Tantalum; Dr. H. C. Bolton has in preparation a Bibliography of the History of Chemistry including Biography and Bibliography; and Dr. Alfred Tuckerman is engaged in indexing the literature to Thermodynamics.

Several bibliographies merit brief mention; Dr. Jesse P. Battershall's Food Adulteration and its Detection (New York, 1887), contains an Appendix with the title:— "Bibliography including Periodicals, Reports and General Works chronologically arranged." This includes about 275 titles. The Second Annual Report of the N. Y. State Dairy Commissioner (1886) contains a Bibliography of Milk by Mr. Edward W. Martin (pp. 156–170), and a Bibliography of Butter, adulterations, testing, etc., by Prof. Elwyn Waller assisted by E. W. Martin and others (pp. 283–290).

Professor Wm. H. Seaman calls the attention of the Committee to several lists of United States Patents which relate more or less to applied chemistry. These lists on subjects indicated by their titles are found in the following works: Charles Thomas Davis, Manufacture of Leather; the same, Practical Treatise on the manufacture of Bricks, Tiles and Terra Cotta; the same, Treatise on Steam Boiler Incrustations; the same, Practical Treatise on the Manufacture of paper; Wm. T. Brannt, Treatise on Animal and Vegetable Fats and Oils. These are all published in Philadelphia, 1884–1887.

B. Tollens' Handbuch der Kohlenhydrate, Breslau, 1888, contains about 1500 references to the literature of carbohydrates.

Dr. Albert Brown Lyons publishes in the Pharmaceutical Era, under the title "Index Pharmaceuticus," a monthly list of books on pharmacy, chemistry and materia medica, as well as a list of original papers on these topics published in journals.

At the meeting of the British Association for the Advancement of Science held in 1886, a Committee was appointed for the purpose of reporting on the Bibliography of Solution. This Committee consists of Professors Tilden, McLeod, Pickering, Ramsay, Young, A. R. Leeds and Nicol (secretary), and presented its first report in 1887. This report sets forth the classification adopted,

the list of journals (thirty-four in number) desirable to index, and a summary of work accomplished. From this summary it appears that 355 titles have been catalogued from 588 volumes of eleven different periodicals. The Committee of the B. A. recommends as members of the Committee other gentlemen who have access to the journals on the list and who would be willing to take an active share in the work.

The Committee of the American Association expresses its gratification that the work begun by them in 1882 is now being supplemented by chemists in Great Britain.

Persons desiring copies of reports, indexes and other information should address the chairman, care of the Smithsonian Institution, Washington.

H. CARRINGTON BOLTON, *Chairman.*
F. W. CLARKE,
ALBERT R. LEEDS,
ALEXIS A. JULIEN,
JOHN W. LANGLEY,
SAMUEL H. SCUDDER,
CHAS. K. WEAD,
Committee.

Report of the Committee on Physics-teaching.

At the Philadelphia meeting of the Association in 1884, the undersigned were appointed a committee to consider and report upon the subject of Physics-teaching.

Brief reports of progress were made at the various meetings since that date, but it is only within the past year that the committee has been able to formulate and agree upon a final report which it now offers to the Association through the Council, at the same time respectfully requesting that it be discharged.

Before entering upon the consideration of the report, it seems proper to refer to some of the causes which have led to so long a delay in its preparation. Shortly after the appointment of your committee, consultation and correspondence with persons interested in the subject developed the great desirability of securing, if possible, the coöperation of the National Educational Association, many of the members of which were more deeply interested if possible, in certain phases of the subject to be considered, than the members of our own body. In accordance with this idea the council of that association, at its meeting in 1885, appointed a committee on Physics-teaching, consisting of Charles K. Wead, LeRoy C. Cooley, W. LeConte Stevens, W. F. Bradbury and James H. Baker.

This committee was not appointed for the specific purpose of coöperating with that of the A. A. A. S. in the preparation of a joint report, but an effort was made by the respective chairmen to secure a joint meeting of the two committees for this purpose.

This effort failed on account of the wide geographical distribution of the members. The committee of the National Educational Association prepared its report, however, and it was presented at the meeting of that body in 1887. It is probably well known to those who have especially interested themselves in this matter.

An attempt was made to secure a meeting of your committee at Buffalo during the meeting of the Association in that city in 1886. A majority of the committee was present and an informal discussion of the subject was had, but it was thought best to defer any report until a full meeting could be held.

Accordingly a meeting was called at Washington, D. C., in December, 1887, and at this meeting, the first session of which was on December 24, all of the members of the committee were present except Professor Trowbridge, who found it impossible to attend.

At this meeting the subject was fully discussed and it was found that the members present were substantially in agreement as to the principal questions involved.

In presenting the conclusions reached it is not thought to be necessary or desirable to insist upon the importance of the study of physics or to offer arguments in favor of its introduction. This ground has been gone over so often and so thoroughly within the past decade that further discussion seems unnecessary.

As a matter of fact it may be said that nearly everything which can be justly claimed is now nearly everywhere admitted. Neither has it been thought necessary to go into detail as to methods of instruction.

The publication in the English language within a few years, of several excellent text-books of physics and a few laboratory guides of a high order of merit, together with a considerable advance in real scholarship among teachers, makes it possible to use the phrases "text-book work," "lecture work" and "laboratory practice" with a fair chance of being understood; yet it may be well to remark that where the latter is referred to, something very different from mere illustrative experimentation is meant, it being the opinion of the committee that the work in the laboratory should be quantitative rather than qualitative and always of as high a degree of precision as is possible with the appliances available.

In order to give definiteness to its conclusions the committee undertook to answer the following questions:—

1. In what grade of the public school should physics-teaching begin?

2. What should be the character of this first instruction? oral? by text-book? by laboratory methods? etc.

3. What should be the character of the physics-teaching in the

high school? text-book? laboratory? text-book followed by laboratory? laboratory followed by text-book? or laboratory and text-book combined?

4. What knowledge of physics should be required for admission to college?

5. What should be the minimum course in physics for undergraduate students and what should be the nature of this course?

In answering these questions the needs of special students, or the requirements of scientific and technical schools and courses have not been considered. It may be fairly assumed that such students and courses and schools will take care of themselves.

The conclusions reached have reference only to the minimum training allowable in a course of study so adjusted as to give the student what we may continue to call, for want of a better name, "a liberal education."

1. In answer to the first question, it is the opinion of the committee that instruction in physics may begin, with profit, in what is generally known as the "grammar school." At the same time it is decidedly opposed to any general recommendation that it must begin there or in the primary school. Here, perhaps more than anywhere else, nearly everything depends on the teacher. One who has a strong liking for and a good knowledge of physics will be tolerably certain to succeed, while another not thus equipped for the work is equally certain to fail. Teachers belonging to the first class constitute an extremely small percentage of the grand total. In science-teaching in grades below the high school, much should be left to the individuality of the teacher. As a result of personal taste or previous training and study, one may give elementary instruction in botany, or in geology or in physiology so as to be a real inspiration to his class, while his instruction in physics might be so intolerably poor as to be unprofitable in the highest degree. The prevailing custom of many public schools which requires all teachers of a certain grade to teach physics is greatly to be regretted and every effort should be made to show school superintendents that it is a mistake which cannot be too quickly remedied.

The rapid advancement which is constantly being made in real scholarship among public school teachers will result in an increased and increasing number of those who are competent to teach physics; and while the committee is convinced that, as a means of real,

honest, mental discipline, no branch of natural science is superior to physics, it would deprecate its forced introduction into the grammar school under circumstances likely to prove disastrous to the best interests of the science.

2. When taught in the grammar school, and by a competent teacher, it should be done mainly by and through illustrative experiments.

These may be of the simplest character, involving and exhibiting some of the fundamental principles of the science, and they should generally be made by the teacher, the pupil being encouraged to repeat, to vary and to extend. Habits of observation and of thought should be cultivated and such facts of the science as are based on or relate to the principles illustrated and developed should be presented. It is neither desirable nor necessary that any particular order should be followed in presenting the various divisions of the subject. The teacher should be guided by circumstances, such as the means at his disposal for experiment and illustration, and often by his own taste and predilection.

The ease with which apparatus for the illustration of the most important principles of physics can be improvised, even when the stock of materials at hand is very slender, puts the science in the front rank as to availability, and it is especially adapted to the requirements of certain schools both in town and country which, through their situation and surroundings, are restricted in their choice of a science subject. If to these facts we add another, which is universally admitted, that the physical properties of matter are the first to be recognized, the laws relating to which being, therefore, the first to arrest attention, it needs no argument to show that a competent instructor will find the study of physics one of the most important educational forces, even in the grammar school.

3. In any discussion of the character of instruction in physics in the high school, one fact of the utmost importance must not be lost sight of. It is that a large majority of the young people who are educated in the public schools receive their final scholastic training in the high school.

Its course of study must be in harmony with this fact, such provision as may be made for those who continue their studies in college or university, being merely incidental.

The high school course in physics must include therefore a general treatment, which must of necessity be elementary in its character, of all the great divisions of the science.

It is likewise important that the student should be made acquainted, if only to a limited extent, with the methods of physical investigation and that he should be able himself to plan and carry out an attack upon some of the simpler problems of the science. The value of this work as an educational factor cannot be overestimated; it is the "walking alone" of intellectual infancy.

It is believed that these two very desirable ends can be reached without giving an undue share of the time and energy of the pupil to the subject. Assuming the high school course to consist of four years of three terms each, it is recommended that the study of physics should begin not earlier than with the third year; that it should continue through one year, three hours a week being devoted to it, not including the time necessary for the preparation of the lesson; and that during the first two terms the work should be text-book work, accompanied by illustrative experiments performed by the instructor and made as complete as his facilities will allow, while the last term should be devoted to simple laboratory exercises. It is hardly necessary to say that during the last term the three hours per week should be grouped into one exercise whenever possible.

Of the character of this laboratory practice it may be well to say that no attempt should be made to carry the pupil through a very great range of subjects. The end sought for can best be reached by a careful and more exhaustive study of a few problems which should be solved with the highest degree of accuracy attainable under the circumstances. As far as possible the pupil should be led to read and study books and papers bearing upon the particular subject which he has in hand. The time demanded by this plan, three hours per week for one school year, barely more than a hundred hours in all, is thought to be the least which is likely to produce results at all satisfactory, and it is urged that a vastly better arrangement is to allow the study of physics to run through two school years, giving it in time, the equivalent of five hours per week for one year.

It is well known that many teachers of physics, and many more who are not teachers of physics, insist on the introduction of lab-

oratory practice from the beginning, some even going so far as to claim that the use of the text-book may be entirely dispensed with. Without desiring to enter into a discussion of this question, we wish to express, and with emphasis, our belief that laboratory practice is in general of little real use to the student unless he comes to it fairly well grounded in the fundamental principles of the science. The somewhat widespread opinion and practice, to the contrary, will be found, it is thought, to be one of those mistakes in which pedagogics seems to be caught on the rebound from other and generally more serious errors.

4. As to the requirements in physics for admission to college, it is sufficient to say that the course indicated above should be required for admission to any and all courses in the college.

5. In reference to the minimum course in physics for undergraduate students in the college, it seems important to avoid the mistake of asking too much.

In many institutions, and especially where the elective system largely prevails, it is possible at present for students to receive a degree and yet be almost absolutely ignorant of the principles of physics. It is the judgment of the Committee that a knowledge of this subject constitutes one of the necessary and essential elements of a liberal education, and a minimum course of three hours per week for one year is recommended. What is usually known as the junior year is most desirable for this work, as at that time the student is sufficiently mature and has acquired the necessary training in mathematics to enable him to make the best of what he does. It is recommended that this course consist entirely of text-book and recitation work, with lectures fully and completely illustrated on the professor's table.

It may possibly excite surprise that laboratory practice is omitted, but it will be remembered that this is a minimum course, in which it is not believed that the laboratory can be made to play a useful part. Whenever it is possible to increase the time devoted to the subject, this course may be and will be followed by a series of laboratory exercises. The nature of these must depend almost entirely on the skill and judgment of the professor in charge and on the facilities which he is able to command.

In any event it should be his aim to attend rather to quality than to quantity of work, and it should not be considered necessary for

each student to work in or even to touch *all* of the great divisions of the subject.

In conclusion, your committee believes that whenever the Association speaks and in whatever it may say upon a subject so important as that under consideration, it should be governed by a wise conservatism, and that it should keep always in view the fact that when its suggestions and recommendations are in the real interests of the school and the college they are certain to be in the line of the advancement of science.

T. C. MENDENHALL, *Chairman*,
WM. A. ANTHONY,
H. S. CARHART,
F. H. SMITH,
Committee.

Report of the Committee on the Preservation of Archæologic Remains on the Public Lands.

The Committee to memorialize Congress for the preservation of Archæologic remains upon the Public Domain has the honor to report as follows:—

After consultation it was agreed that it would be well if the following remains of early America could be preserved:—

Chaco Cañon, from the forks of Escavada cañon for a distance of eight miles up; also one mile back from the brink of the cañon walls on each side so as to include many interesting structures thereon.

Cañon DeChelly; Cañon Del Muerto; Walnut Cañon; The Ruin on Fossil Creek, an east branch of the Rio Verde, and about fifteen miles south of Camp Verde Military reservation; Ruins in Mancos Cañon; the Round Towers situated on the flat valleys of the lower Mancos.

The Cavate Lodges in the cinder cone, about eight miles east of Flagstaff, Arizona. Beside these groups of ruins, and dwellings, there are isolated remains in the territories of New Mexico, Arizona and Utah, numbering over forty, which demand preservation.

The Pueblos, which are not on treaty reservations, or grants; and the old Mandan and Arickaree village on the Fort Berthold Indian reservation, Dakota Territory, to be preserved, when they shall cease to be inhabited by the Indians. Also certain burial and village sites in Alaska.

Although every courtesy and assistance was rendered the Committee by the Geological Survey, it was found that field work would be required in every case but one, in order properly to designate the exact acreage needful to be set aside by Congress for the effectual protection of each ruin or group of ruins. This field work would require time, possibly two seasons, and involve expense.

In order to meet this difficulty and to inaugurate the precedent of preserving archæologic remains upon the Public Domain, it was thought best to prepare a bill, which should reserve the one tract, the boundaries of which were already ascertained, and to direct a report upon other tracts needful for the preservation of archæologic remains to be made in the future to Congress by the Director of the Geological Survey. Heretofore there has been but one instance of legislation for the protection of the monuments of ancient America, that of the Legislature of Ohio, in reference to the Serpent Mound in that State. This relic including a tract of seventy acres has been bought and placed in the charge of the Trustees of the Peabody Museum of American Archæology and Ethnology of Harvard University, Mass.; and to Frederick W. Putnam, its curator, and Professor of American Archæology, is due the honor of making this first movement in behalf of American Archæologic culture.

The tract of land with ascertained boundaries, which Congress is asked to reserve, is situated on the Rio Grande, west of Sante Fé, New Mexico. It contains groups of cavate dwellings cut in the tufa, which show three distinct ages of occupancy: the two earlier ones dating back to an unknown period; the latest, it is thought, took place during the Spanish Wars of the sixteenth and seventeenth centuries. Upon the top of the walls of the cañons, stone pueblos were erected; these, however, have mostly fallen to ruins. The entire group is an interesting series of dwellings, both excavated and built, and is well worth preservation and study. Your committee have accordingly introduced the following bill into Congress with the promise of its speedy passage through both Houses:

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the tract of land in the Territory of New Mexico described as follows: beginning at the northwest corner of Cochiti Indian grant, and running north to the southeast corner of "Baca Location, No. 1;" thence east to the Ramon Vicil grant; thence southeasterly along such boundary to the eastern line of Bernallillo County; thence southward to a point directly east of the place of beginning; thence due west to place of beginning, is hereby reserved and withdrawn from settlement, occupancy, or sale under the laws of the United States; and all persons who shall locate or settle upon or occupy the same, or any part thereof, shall be considered trespassers and removed therefrom.

SEC. 2. That said Reservation shall be under the exclusive control of the Secretary of the Interior, whose duty it shall be, as soon as practicable, to make and publish such rules and regulations as he may deem necessary or proper for the care and management of the same. Such regulations shall provide for the preservation from injury and spoliation all natural and archæological curiosities within said Reservation and their retention in their present condition.

SEC. 3. The Director of the Geological Survey is hereby directed to make a report to Congress specifying such other archæological remains existing upon the Public Domain, as should be preserved in the interests of science, together with a description of such tracts of land as it may be necessary to reserve in order to insure the protection of said archæologic remains from injury and spoliation.

Hoping that the efforts of the committee in behalf of Archæologic study may meet the approval of the Association,

Respectfully submitted,

ALICE C. FLETCHER,
T. E. STEVENSON,
Committee.

Washington, D. C., July 18, 1888.

Report of the Committee to secure from Congress the Abolition of the Duties on Scientific Books and Apparatus imported into this country.

The committee makes the following report:

Shortly after its appointment Dr. J. S. Billings resigned, suggesting that a member from the West be selected to fill his place. Professor Langley, president of the Association, therefore appointed Prof. A. H. Worthen of Springfield, Ill., to the vacancy. Upon the death of Professor Worthen, which followed not long after, Prof. S. A. Forbes of Champaign, Ill., was selected by Professor Langley to fill his place.

The eastern members of the committee, Prof. J. R. Eastman and Prof. E. D. Cope, chairman, have held several meetings with the following results. The following resolution was adopted and signed by all the members:

Resolved: That there shall be admitted to this country, free of duty, all books in languages other than English; of books in the English language all single copies sent to periodicals issued not oftener than once a week; all books issued by governments and scientific societies and all other books not republished within a year after the first publication in an English-speaking country.

Also all apparatus, instruments and material to be used in scientific experiment or original research; decision as to the intention of the importer to rest with a committee of the U. S. National Academy of Sciences.

This resolution was placed in the hands of the Committee of Ways and Means of the House of Representatives of which the Hon. R. Q. Mills is chairman, through Hon. W. C. P. Breckinridge of Kentucky, one of its members. The resolutions were received with consideration and had the attention of the committee while engaged in framing what is known as the Mills Tariff Bill. The recommendations contained in the resolutions were partially incorporated into the bill in the following language (H. R. 9051, p. 1 and 7).

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, that on and after the first day

of July, 1888, the following articles mentioned in this section, when imported, shall be exempt from duty:

"Bibles, books and pamphlets printed in other languages than English, and books and pamphlets and all publications of Foreign Governments, and publications of Foreign Societies, historical or scientific, printed for gratuitous distribution."

This provision, although not covering the case of scientific publications produced in England, is a great advance over previous legislation on the subject.

As it is probable that the Mills bill in its present shape will not pass the Senate, and as the Senate is preparing a tariff bill as a substitute for it, the resolutions of your committee have been submitted to the committee of the Senate engaged in preparing this bill, of which Senator W. B. Allison is chairman. There is every reason to believe that the suggestions therein contained will receive the respectful consideration of that committee.

It has been hoped that some relief from the tax on knowledge at present imposed by the government of the United States might be largely relieved by the operation of the bill which has passed both houses of Congress, known as the Chace copyright bill. Such relief would really result, in the case of scientific books of which the sale should be sufficiently large to justify their republication in this country by foreign publishers, since they would then cease to be imported; but, unfortunately, the books most needed by students engaged in original research in this country are generally of a kind that are not republished, owing to the limited demand for them relatively to other classes of publications.

By resolution of the American Society of Naturalists, your committee has been made the recipient of the following preambles and resolutions and its request for the presentation of the same to Congress:

"Whereas, the cause of education in science is retarded by the restrictions placed by Congress on the importation of scientific books and apparatus: whereas we believe that advance in the arts and industries depends on the development of science and is impeded by the before-mentioned import duties, and that all restrictions on education and scientific research are unworthy of enlightened government: whereas the scientific books published abroad are absolutely essential to students and investigators, and are but rarely duplicated in this country: whereas the value of scientific apparatus is in nearly all cases dependent on the individuality of the maker: and whereas colleges and incorporated institutions are now permitted to import apparatus duty free, while private investigators, usually less able to afford expense, are obliged to pay duty, therefore

Be it resolved, That ———— hereby requests the Representatives of the state of ——— in the Congress of the United States to use all possible efforts to have placed on the free list, books pertaining to the physical, natural and medical sciences, and apparatus intended for purposes of scientific research or of education; and further be it

Resolved, That a copy of these preambles and resolutions be forwarded to each member of Congress."

These resolutions were sent, by resolution of the same society, to the faculties of the universities and colleges throughout the United States for their approval and signature. Replies expressing this approval and signed by the faculties, or their accredited officers, were received from the following institutions:

The American Philosophical Society, Philadelphia.
The Franklin Institute, Philadelphia.
The Academy of Natural Sciences, Philadelphia.
The County Medical Society, Philadelphia.
The Society of Arts, Boston.
Brown University, Providence, R. I.
Bryn Mawr College, Pa.
Wellesley College, Mass.
Iowa College, Iowa.
Michigan University, Ann Arbor, Mich.
Wesleyan University, Middletown, Conn.
Naval Academy, Annapolis, Md.
Middlebury College, Vt.
Hamilton College, Clinton, N. Y.
Swarthmore College, Pa.
Adelbert College, Ohio.
Williams College, Williamstown, Mass.
College of the City of New York.
University of Alabama, Tuscaloosa, Ala.
Lafayette College, Easton, Pa.
Amherst Agricultural College, Amherst, Mass.
Haverford College, Pa.
Smith College, Mass.
Columbian University, Washington, D. C.
Stevens Institute of Technology, Hoboken, N. J.
University of Indiana, Indiana.
Lehigh University, Bethlehem, Pa.
University of California, Berkeley, Cal.
Hobart College, New York.
College of Physicians and Surgeons, New York City.
University of North Carolina, N. C.
Columbia College School of Mines, New York City.
Union College, New York.
Kenyon College, Ohio.

Northwestern University, Evanston, Ill.
Marietta College, Ohio.
University of Virginia, Charlottesville, Va.
Cornell University, Ithaca, N. Y.
Hampden Sidney College, Va.

As regards the removal of duty from imported philosophical and scientific apparatus, your committee cannot report much progress. Our efforts have been mainly directed to the removal of the duty on books, under the belief that success in this direction will prepare the way for further progress. We have not, however, neglected this important subject. The Mills tariff bill thus refers to it (pp. 27 and 32):

(p. 32.) "And on and after October first, 1888, in lieu of the duties heretofore imposed on the articles hereinafter mentioned in this section, there shall be levied, collected and paid the following rates of duty on said articles severally:"

"Philosophical apparatus and instruments, twenty-five per centum ad valorem."

Your committee hope to be able to secure the total abolition of the duties on foreign books of science, and the great reduction, if not abolition, of those on apparatus. We base this hope on the activity in the direction of change in the existing laws on this subject, at present existing in Congress, and the evident desire of the representatives of both the great political parties of the country to legislate for the best interests of their constituents, as they understand them. At the moment of preparing this report it is not possible to announce any final result of the action of your committee, but it is quite possible that improved legislation may be attained by the time of the meeting of the Association to which this report is made.

In conclusion we feel that what is needed to effect the result desired, is a continuation of the effort, already commenced, of vigorous protest against the laws on the subject as at present existing; laws which obstruct knowledge at its fountain-head; which impose onerous burdens on a class which works gratuitously for the public good, and which place our country in a false position among the enlightened nations of the earth.

EDW. D. COPE, Philadelphia, *Chairman*,
J. R. EASTMAN, U. S. Naval Observatory, Washington,
S. A. FORBES, Champaign, Ill.,
Committee.

Report of the Committee on Anatomical Nomenclature with special reference to the Brain.

With regard to encephalic nomenclature, the committee understand that three distinct tasks are to be performed:

A. The preparation of a list of those names which they regard as worthy of adoption; the terms to be in Latin, with appropriate equivalents (etymologically similar so far as possible), in English, French, German and Italian.

B. The formulation of comprehensive principles of anatomical nomenclature, and of rules that should govern the selection of existing terms and the formation of new ones.

C. The recommendation of descriptive terms to be used in indicating the position and direction of encephalic parts with respect to one another and the longitudinal axis.

These tasks are both extensive and difficult, and other and imperative duties have prevented the committee from devoting to them as much time as they had hoped; moreover, their attention has been called quite recently to certain publications in which the principles of anatomical nomenclature are somewhat fully discussed but which it has not been possible to examine as yet with the care which they deserve.

The committee therefore ask to be continued, and hope to have made visible progress by the time of the next meeting of the Association.

Burt G. Wilder, *Chairman*,
Harrison Allen,
Frank Baker,
T. B. Stowell,
Committee.

Note.—The undersigned was out of the country at the time the above report was prepared and submitted, but now expresses his concurrence therein.

Henry Fairfield Osborn.

REPORT OF COMMITTEE ON INTERNATIONAL CONGRESS OF GEOLOGISTS.

THE committee on the International Geological Congress report that the papers prepared and brought to the Association at its meeting last year have been transmitted to the Congress, which is about to assemble in London. Their duties in this respect are done. The treasurer, Prof. H. S. Williams, reports that all the expenses incurred by the committee are paid and the accounts closed.

The secretary, Dr. Persifor Frazer, has in hand a subscription list for copies of the geological map of Europe, prepared by the International Geological Congress. One hundred copies are proposed to be subscribed for here and it is reported that ninety-four subscribers are secured.

The invitation for the Congress to hold its next session, that of 1891, in this country will be presented at the coming session of that Congress. With these duties performed the functions of the committee will end, and future business of the International Congress devolves upon the A. A. A. S. and the American Geological Society, which it is expected will act with Section E of this Association.

Report presented by

GEORGE H. COOK.

Report of Special Agent of Transportation for the Association to act with the Local Committees.

In regard to the efforts to secure reduced fares for members attending the meeting: — The membership of the A. A. A. S. being distributed over so large an extent of country several distinct Passenger Agents' Associations are included therein, each of which require separate negotiations in obtaining concessions. The rules and regulations of the associations are unlike, therefore unity and uniformity of action cannot be obtained of all, especially from those most distant from the place of meeting.

The rules of the principal associations are now such that the chairman of the association can, under definite limitations, grant concessions for fares without submitting the question to a vote of the passenger agents of the association.

The A. A. A. S. can feel reasonably certain of obtaining a concession in the Passenger Association where the meeting is held, also in the adjacent associations, of a fare and one-third for the round trip, on the certificate plan and three days' limit.

The requirements of such a concession are that one hundred members attend the meeting from two of the associations. This does not meet all cases, but is the best which can be obtained without a special vote by the different Passenger Agents' Associations, which is no easy matter to obtain.

For the Cleveland meeting I applied to the principal associations for a concession of our fare for the round trip and an extension of the three days' limit. Neither was granted.

Before the associations will take any official action for a reduction of rates, an application must be filed stating the name of the organization, place of meeting, date of meeting, the probable number in attendance and the name of the secretary who will sign the certificates.

All of this information can be furnished by the agent except the name of the secretary who is to sign the certificates.

The name of the latter should be designated by the Local Committee for the next meeting by the first of January, 1889.

The date of the meeting should be counted from the time the council meets and adjourns.

P. H. Dudley, *Honorary Agent.*

SECTION A.

MATHEMATICS AND ASTRONOMY.

OFFICERS OF SECTION A.

Vice President.

ORMOND STONE of University of Virginia, Va.

Secretary.

C. L. DOOLITTLE of Bethlehem, Pa.

Member of Council.

WILLIAM HARKNESS of Washington.

Members of Sectional Committee.

H. A. NEWTON of New Haven, Conn., HENRY M. PAUL of Washington, R. S. WOODWARD of Washington.

Member of Nominating Committee.

E. W. HYDE of Cincinnati, Ohio.

Members of Sub-committee on Nominations.

G. C. COMSTOCK of Madison, Wis., ALVAN G. CLARK of Cambridge, Mass., C. H. ROCKWELL of Tarrytown, N. Y.

ADDRESS

BY

PROFESSOR ORMOND STONE,

VICE PRESIDENT, SECTION A.

MOTIONS OF THE SOLAR SYSTEM.

As in the moral world unselfishness stands high above all other virtues, so in science the interest in any subject depends principally upon its relation to other subjects. Thus pure mathematics derives its chief value from its applications or from the possibility of application. Astronomy affords the highest opportunities for the exercise of genius in the development of geometrical analysis. But its real interest does not centre in the analysis itself, but in its application to the study of the motions of the physical universe; nor is it the motions themselves which excite our chief interest, but rather those philosophical questions which relate to the origin, structure and unity of the universe.

No other hypothesis has been suggested which offers such direct and complete answers to most of these questions as Newton's law of gravity. It is but natural, therefore, that the majority of the problems which arise in regard to the motions of the solar system should have their origin in an effort to confirm this law, so remarkable for its wonderful simplicity, yet so complicated in its consequences, that many of the most important problems arising therefrom remain still unsolved.

The first attempt to apply Newton's law to all the motions of the solar system was made by Laplace, and to-day there exists no work in science which for boldness of design, grandeur of outline,

and success of accomplishment, surpasses the *Mécanique Céleste.* Nevertheless, when Lindenau and Bouvard undertook to compute their tables of the motions of the planets, a complete revision of Laplace's theory was found necessary. This was due to the fact that Lindenau and Bouvard had more numerous and more accurate observations with which to compare Laplace's theory than the latter had employed in its formation. Every advance in accuracy of observation demands a corresponding advance in theory.

As no general solution of the problem of more than two bodies has thus far been obtained in finite terms, in any analytical discussion of the motions of any dynamic system, it is necessary to resort to the use of infinite converging series. Now, it is a peculiarity of such series, that the expressions involving small quantities of any given order of magnitude are generally more complicated than those of a lower order, although in general the former are much smaller than the latter. Mr. Hill, in speaking of his computation of the inequalities of Jupiter and Saturn, especially mentions that more time was consumed in computing terms of three dimensions than in computing those of two. On the other hand, every additional observation adds to the work of the computer. Such are some of the difficulties which beset the computer in his efforts to obtain numerical solutions of problems in celestial mechanics.

So enormous, indeed, is the labor involved, that there exists besides those mentioned only one other complete set of theories and tables of the motions of the principal planets, that of Leverrier. Leverrier's tables of the inner planets are now nearly thirty years old, and those in use to-day, if we except Hill's tables of Venus, which were published in 1872, in which Leverrier's theory was employed, but corrected by comparison with observations. Leverrier's tables of the outer planets are much later, having engaged his attention almost to the day of his death. His tables of Jupiter and Saturn were published in 1876 and those of Uranus and Neptune during the following year. Bouvard's tables of Jupiter and Saturn preceded those of Leverrier no less than fifty-five years.

Newcomb's tables of Neptune were published in 1865, those of Uranus in 1874. Hill's Theory of Jupiter and Saturn which has for years occupied his attention has at last been completed, and he is now engaged in preparing tables therefrom. These are intended to form a part of a complete series of tables of the principal planets now being prepared under the direction of Professor New-

comb, at Washington. Another such series is also being prepared by Professor Gyldén, at Stockholm.

The values of the coëfficients of the terms of short period in the motions of the principal planets are now pretty well known; and the same might be said of the secular variations, were it not for the difference between theory and observation which exists in regard to the motion of the perihelion of Mercury which was discovered by Leverrier, and has been confirmed by Newcomb in a discussion of the observations of the transits of Mercury extending over a period of more than two centuries. The cause of this difference still remains unknown. Whether the law of gravity requires modification, or the difference is produced by the attraction of unknown matter in the neighborhood of the sun or Mercury, or whether some force other than gravity effects the motion of Mercury without being appreciable in the case of the planets more remote from the sun, we cannot tell. There is evidently no planet or group of planets as Leverrier supposed between Mercury and the sun; for, although careful search has been made during recent eclipses in the hope of discovering one or more such bodies, it has always been in vain. The objects seen by Watson and Swift during the eclipse of 1878 were undoubtedly fixed stars. The claim has, indeed, been made that such bodies had been observed crossing the sun's disc, but no such phenomenon has been seen by any skilled observer. Professor Newcomb thinks that even disintegrated matter in sufficient quantity to produce the effect under consideration would shine more brightly than the zodiacal light, and, unless situated near the plane of Mercury's orbit, would produce a secular variation of the node which does not exist. Tisserand has computed the secular variation of the planets employing Weber's Electrodynamic law of gravity, but the difference of the value of the motion of the perihelion of Mercury thus obtained from that given by Newton's law is much smaller than the discordance derived from observation. The completion and comparison with observations of the new theory of the four inner planets now being prepared under the direction of Professor Newcomb will be awaited with interest with the hope that it may throw new light upon this interesting subject.

Of the satellites of the solar system our moon has naturally attracted more attention than all the others combined. The only recent original tables of the moon's motions are those of Hansen.

These, like Leverrier's tables of the inner planets, are now more than thirty years old. These tables have been compared with observations, and agree fairly well with those made during the century preceding their publication, but not with those made before or since that time.

Halley was the first to call attention to the existence of a secular acceleration of the moon's longitude. Laplace made a numerical application of Lagrange's theory of secular variation, and thus explained the lunar acceleration on theoretical grounds. Adams, however, was the first to obtain a practically correct theoretical value of this acceleration (6″), which has since been confirmed by Delaunay and others. Hansen, on the other hand, by a comparison of his Lunar Theory with historical statements which he assumed to refer to certain ancient total eclipses of the sun, concludes that the acceleration is 12″, or twice that found by Adams. Professor Newcomb, on the other hand, considers the employment of these eclipses for the determination of the secular variation as entirely unwarranted. If historical statements of the ordinary events of those times are unreliable, especially so must be statements in regard to scientific phenomena made by unscientific men, and often given at second-hand. Even if we assume in any given case that the statement refers to a total eclipse of the sun, the locality where the eclipse was total is nearly always extremely uncertain. Accordingly, Professor Newcomb, in determining the value of the acceleration, limits himself to two sources of evidence, the Ptolemaic eclipses of the *Almagest*, and the Arabian eclipses contained in *Le Livre de la Grande Table Hakémite*. From these he concludes that we cannot suppose the acceleration much greater than 8″.3 without supposing systematic errors which seem quite improbable. In fact, if the Arabian observations, which are more accurate than those of Ptolemy, be alone combined with modern observations, the result is only 7″, a value but little greater than the theoretical value.

Dr. Ginzel has made an extensive comparison of Hansen's theory with accounts of total eclipses of the sun of the middle ages which have been found in the many cloisters scattered over the continent of Europe. From these accounts he has estimated the differences between the distances north or south, in degrees of latitude, of the average places of observation from the lines of central totality as obtained from Oppolzer's eclipse tables, which are based

upon Hansen's theory. From these differences of latitude he has formed equations of condition from which to obtain corrections to the coëfficient of acceleration and the mean motions in longitude, argument of latitude, and mean anomaly. Having solved his equations of condition, he finds that certain ancient eclipses are also fairly well represented by the corrections obtained. He, accordingly, includes these in a new set of equations, rejecting, however, two of the later eclipses which in the first solution had given the largest residuals. The solution of this second set of equations gave him a second set of corrections much smaller than those first obtained and representing the old eclipses much better, and finally concludes that Hansen's value of the acceleration requires a change of only a little more than 1″. A careful inspection, however, of his residuals shows that these are almost exactly identical in both solutions for those eclipses which are common to both; whence, it may be concluded that his solution depends in reality upon the ancient eclipses alone, which in turn should be accepted with caution, or perhaps rejected entirely on the grounds so ably discussed by Professor Newcomb. From the evidence we have, it seems probable, though not positive, that a small secular residual still remains unexplained.

Several hypotheses have been suggested in explanation of the supposed discordance between the observed and theoretical values of the acceleration. In a complete theory of the moon, there should exist no terms which increase with the time, but in place of the so-called secular terms, there should be terms of very long period; but the determination of these terms of very long period is a problem as yet unsolved. Still, it would hardly seem probable that there exists any term of this character not taken account of during the few centuries over which observations have extended, of sufficient size to account for the large difference assumed by Hansen. Another hypothesis is the assumption that the earth is changing the velocity of its rotation on its axis either by reason of the friction produced by the tides of the ocean, as originally suggested by Kant and afterwards revived by Mayer, Ferrel, and Delaunay, or by reason of friction produced by bodily tides on the assumption that the earth is partially viscous as assumed by Darwin. The acceleration might also be accounted for by assuming that the law of gravity, as given by Newton, is only a very close approximation to the true law of nature, or by assuming that the motion of the moon is affected by the action of other forces acting independently of grav-

ity. The supposition suggested by Oppolzer that the moon is retarded by the impact of meteors is entirely inadequate, nor is it reasonable to suppose that there exists a resisting medium of sufficient density to produce an appreciable effect on the moon's motion.

An examination of the residuals found by Professor Newcomb in his researches on the motions of the moon would seem to indicate that, aside from the acceleration, there exist appreciable unexplained inequalities of somewhat long period. It hardly seems possible that the rotation of the earth on its axis could be so irregular as to be the cause of these inequalities. We should rather, it seems to me, look for an explanation to a more accurate knowledge of the theory of the moon's motion based upon the law of gravity, or to the existence of other unknown forces.

The only other theory of the moon comparable with Hansen's is that of Delaunay, which forms Vols. XXVIII and XXIX of the Memoirs of the French Academy. The method there adopted is so elegant that in Mr. Hill's opinion it cannot fail to become in the future the classic method of treating all problems of celestial mechanics. This theory, however, is limited to a determination of the inequalities in the motion of the moon due to the action of the sun on the hypothesis that the orbit of the earth is a pure ellipse. Mr. Hill has since determined the inequalities due to the ellipticity of the earth's figure and a portion of the disturbance produced by Jupiter. The subsidiary inequalities, however, due to the inequalities in the earth's motion and to the disturbance of the other planets remain still to be determined.

Delaunay's method differs from that of Hansen in that the inequalities determined are not expressed numerically but only symbolically in terms of arbitrary constants.

The longitude and latitude can be more accurately confirmed by observation than the radius vector, which is not observed directly. Delaunay determined the inequalities of the longitude and latitude to terms of the seventh order, or, when the convergence of the series was found too slow, to terms of the eighth and ninth order. The radius vector, however, he carried only to terms of the fifth order, which is not even sufficient for a determination of the moon's parallax. Professor Adams has, however, since calculated the inequalities of the radius vector of the moon to the same degree of accuracy Delaunay obtained for the latitude and longitude.

A numerical comparison made by Professor Newcomb between

Hansen's and Delaunay's theories shows that the solar terms of short period are now fairly well known. In view of the magnitude of the work involved in a determination of the motion of the moon, it is remarkable how few errors have been detected in the work of these masters of celestial mechanics. So far as he has gone, Delaunay's theory is, so far as I am aware, absolutely perfect. This cannot of course be said in regard to Hansen's, since his operations are all numerical, and therefore subject to inaccuracies arising from an imperfect knowledge of the values of the constants employed. A few additional errors have also been detected. Hansen assumed that the centre of gravity of the moon does not agree with the centre of the figure, an apparently unwarranted hypothesis. The coefficient of the largest inequality thus introduced is, however, only one-third of a second. A small inequality was accidentally introduced into the tables with the wrong sign, and one other more important term has been found to have no theoretical existence. While the coëfficients of the inequalities upon which Hansen's tables are based seem to be pretty well known, I am not aware that the tables themselves have been sufficiently checked except by comparison with observations. Apparently, the great desideratum now is a set of tables computed from Delaunay's theory in a completed form, or computed in some other way absolutely independently of Hansen's. Until Hansen's tables are thus checked, it is questionable whether it can be safely said that the motion of the moon cannot be completely accounted for by the law of gravity.

Among the many important investigations in regard to the moon's motion, not already referred to, may be mentioned Dr. Hill's remarkable researches, especially his memoir on the motion of the lunar perigee, and Professor Adams's paper on the motion of the moon's node. In these, instead of first determining inequalities of the first order in regard to the mass of the disturbing body, the terms which are independent of the eccentricities and inclination are first obtained; and afterwards those which involve the first powers of the eccentricities and the sine of the inclination; and so on.

Probably the greatest very recent loss to astronomy was sustained in the death of Oppolzer. The remarkable career of this distinguished astronomer during the past quarter of a century inspired the hope that the researches in the lunar theory which he had only just entered upon at the time of his death would result in a veritable addition to our knowledge of this subject.

The detection of the two satellites of Mars by Professor Hall with the great equatorial at Washington may be considered the most interesting recent achievement in pure discovery. Phobos, the inner of these, is the only satellite in the solar system which revolves about its primary in less time than it takes the primary to rotate on its axis, and hence the only satellite which rises in the west and sets in the east.

The accuracy of the values of the masses of the planets obtained from discussions of orbits of comets and asteroids varies greatly according to the size of the inequalities produced. If the principal inequality is large and the body has been carefully observed through a long period by a large number of observers, the result ought to be accurate and practically free from personal error. But the most favorable circumstances seldom occur, and it was not till the discovery of the satellites of Mars that a means was offered for the accurate determination of the mass of this planet. No satellites of Venus and Mercury have as yet been discovered, and the values at present assumed for the masses of those planets are very uncertain.

In 1788, just one hundred years ago, Laplace published a complete theory of Jupiter's satellites. This theory is still the basis of the tables now in use. Souillart's analytical theory of these satellites appeared in 1881. His numerical theory was completed only within the last year, and tables therefrom still remain to be formed.

Titan, the largest satellite of Saturn, was discovered by Huygens in 1655; soon afterwards Cassini added four to the number; a century later two more were added by Herschel; and, finally, Hyperion was discovered by Bond in 1848. Bessel made a careful investigation of the orbit of Titan and therefrom obtained a value for the mass of Saturn which since that time has been generally employed in the determination of the perturbations produced by that planet. The general theory of the Saturnian system which he commenced, he did not live to finish. It may be found in its incomplete condition in the *Nachrichten*. "This memoir," as Professor Hall remarks, "is still the most comprehensive investigation we have of the differential equations of this system and of the various forms of the perturbative function arising from the figure of the planet, the ring, the action of the satellites on each other, and the sun."

Our knowledge of the motions of the satellites of Saturn, with the exception of Titan, was very meagre until quite recently. This system of satellites is, in many respects, the most interesting in the solar system, and its form is quite analogous to that of the primary system. The number of members is the same, and Titan plays very much the same role in the one system that Jupiter does in the other. Since its erection, the great equatorial of the Washington Observatory has been chiefly devoted to observations of the numerous satellites of the outer planets. Professor Hall has published a discussion of his observations of all the satellites of Saturn except Hyperion, but omitting a discussion of the motion of the node of Iapetus. The mass of Saturn obtained by Professor Hall from micrometric measures of position angles and distances is smaller than that obtained by Bessel. The difference is perhaps due to the difference in personal errors in the measurements of the satellites when in different positions with regard to the primary. This error is increased by the fact that when the position of the satellite is compared directly with the primary, one of the micrometer wires must bisect the disc or we must measure from one of the edges of the planet. In the case of Saturn, the difficulty is further increased by the presence of the rings. To avoid these difficulties, Otto Struve has suggested the comparison of the positions of the satellites with one another instead of with the planet. Within the last two or three years Hermann Struve has made such a series of observations, with the great refractor of the Pulkowa Observatory, resulting in a mass of Saturn practically the same as that of Bessel.

A difficulty in the determination of a correct theory of the motions of Saturn's satellites is the fact that there are a number of cases of approximate commensurabilities in the ratios between their so-called mean motions. For example, the mean motion of Iapetus is almost exactly one-fifth that of Titan, Dione one-half that of Enceladus, and Thetus one-half that of Mimas. But the most interesting case is that of Hyperion, whose mean motion is very nearly three-fourths that of Titan. In this case, there is the additional difficulty that their distance from one another is only about one-seventh as great at conjunction as at opposition. The apparent eccentricity of Hyperion's orbit is principally due to the perturbations produced by Titan. As a consequence, Hyperion is always at apo-saturnium when in conjunction with Titan. The values for the mass of Titan obtained from discussions of the motion of Hyperion by Mr. Hill and myself agree closely with one

another, and with that obtained by Hermann Struve from the motion of the node of Iapetus. The values previously obtained by Newcomb and Tisserand are evidently too small.

Our knowledge of the motions of the satellites of Uranus and Neptune depends almost entirely on the observations made at Washington. Quite accurate determinations of the masses of these two planets have been obtained from them. The theory of their satellites offers no other points of special interest. The large secular motion of the plane of Neptune's satellite to which Marth has called attention needs confirmation; so that we may say that with the exception of the lunar acceleration there is little evidence that any of the satellites of the solar system move otherwise than in exact accordance with Newton's law of gravity.

The discovery of the asteroids began with the century. Only four were detected by the end of 1807, and it took forty years to double this number. Since that time, however, their discovery has progressed with ever increasing activity, until the number of these diminutive bodies has reached two hundred and seventy-eight, and is now increasing at the rate of nearly one hundred to every decade. The asteroids individually possess little interest of their own; but, on account of their motions, and on account of the assistance which their observation offers in the solution of other problems, they have already played an important part in the history of astronomy. The discovery of Ceres led to the composition of the *Theoria Motus*, and a demand for a more accurate knowledge of their motions was the origin of Hansen's *Auseinandersetzung*. Several of the asteroids approach Jupiter sufficiently close to offer a valuable means for the determination of the mass of that planet. On the other hand, another of the asteroids is at times actually nearer to the sun than Mars, but unfortunately the inclination of its orbit is very great.

The discovery of new asteroids should be encouraged in the hope that one or more may be found having inequalities in their motions produced by Mars and the earth of sufficient amount to render observations of them valuable for the determination of the mass of Mars and of the earth, the mass of which is, strange to say, not nearly so well known as those of Jupiter and Saturn. The masses of the latter are obtained from the observed distances of their satellites, while in the case of our own satellite the distance is the one quantity which it is impossible to observe.

The number of asteroids is so great that they have been the fre-

quent subject of statistical investigation. Perhaps the most important earlier investigations of this sort were those of d'Arrest and Newcomb, which threw doubt upon the hypothesis of Olbers, that the asteroids were the fragments of an explosion of a larger planet. The systematic grouping of their nodes and perihelia which exists, was shown by Newcomb to be the effect of perturbation. A clearer light has been thrown upon this subject, more recently, by Glauser and Newton. As a result of the action of Jupiter, the orbit of each asteroid has a motion about the orbit of that planet. These motions being unequal, there is a tendency toward a uniform distribution of the nodes on the orbit of Jupiter. Glauser shows that the observed grouping of the nodes on the ecliptic is a subsidiary result of such a uniform distribution, which, however, is slightly disturbed by the action of Saturn. Newton had previously found that the centre of gravity of the poles of two hundred and fifty-one asteroid orbits, considered as points of equal weight, lies within half a degree of the pole of Jupiter's orbit, and in fact regarding this centre of gravity as the pole of the mean plane of the asteroid orbits, that this mean plane lies nearer to the plane of Jupiter's orbit than to the orbit plane of any individual asteroid. On the other hand, if weights be given derived from the observed magnitudes of the asteroids, the position of their mean plane as found by Svedstrup differs greatly from that of Jupiter.

Kirkwood concludes, from an inspection of a table of the mean distances of the asteroids, that those parts of the asteroid zone in which a simple relation of commensurability would obtain between the period of a minor planet and that of Jupiter, are distinguished as gaps or chasms similar to the intervals in Saturn's ring. This much can, at least, be said that eighty-five per cent of the asteroids have mean motions greater than twice and less than three times that of Jupiter, and the mean motions of none approximate closely either of these, the two simplest ratios possible. The next simplest ratios lie beyond the limits of the zone; that is, there are no asteroids having mean motions nearly equal to or less than three halves that of Jupiter, and none nearly equal to or greater than four times that of Jupiter. There are other cases, however, in which the mean motions approximate very closely to commensurabilities which, although more complicated than those just mentioned, are still comparatively simple.

The labor of determining the general perturbations and computing tables of an asteroid is as great as in the case of a major

planet. It is no wonder, therefore, that tables have been prepared for scarce a dozen of these small bodies, and that these are already out of date. An examination of the Berlin *Jahrbuch* will show how much labor is required to determine their motions with sufficient accuracy to identify them, to prevent them from being lost, and to distinguish them from any new ones which may be detected. So far as we are yet aware, their motions are in exact accordance with Newton's law of gravity. Any divergence from that law, if any exist, will require many years, perhaps centuries, of careful and patient investigation to determine.

Many comets being visible to the naked eye, records of them extend back to very early times. As the number of persons interested in astronomy has increased, and especially since the discovery of the telescope, the number of these records has continually enlarged. As, however, the ease with which comets are detected does not, beyond a certain limit, increase with the aperture of the telescope employed, as in the case of asteroids, the rate of their discovery has not increased as rapidly as that of the minor planets. Before 1867 there were only eight periodic comets which had each been observed at at least two appearances. Since that time three comets of short period have been discovered, all by Tempel, which have been observed at at least two appearances. More recently, five comets of short period have been discovered by others, which, with perhaps one exception, had not apparently been observed before, and the time for whose next appearance has not yet arrived. The exception is Findlay's 1886 VII which may be the same as de Vico 1844 I. Quite recently returns have also been observed of two comets of long period, those of Olbers and of Pons.

Of well-known comets of short period Encke's, which has the shortest period of any, possesses the greatest interest to the student of celestial motions, since it was from a discussion of the orbit of this comet that Encke detected evidence of the existence of a resisting medium which produces an acceleration in the comet's mean motion. A more recent investigation by von Asten confirmed Encke's hypothesis so far as the observations from 1819 to 1868 were concerned, but showed that this acceleration did not exist after 1865. Backlund, who took up the problem after the death of von Asten, finds that while the latter result was due to the use of erroneous formulæ in the computation of the terms of the second order for the period 1865 to 1868, the acceleration of the mean motion is nevertheless diminishing. Backlund ends his

memoir by a theoretical consideration of the action of a resisting medium, and concludes that facts agree better on the assumption that the medium acts directly as the velocity, and inversely as the square of the radius vector. The investigations of Oppolzer and Haerdtl indicate that there is an acceleration also in the mean motion of Winnecke's comet. These two are the only comets whose motions so far as they are known cannot be fully satisfied by Newton's law. The large eccentricities of the orbits of periodic comets have thus far rendered it impracticable to compute tables of their motions as has been done in the case of planets and their satellites. The general perturbations of Encke's comet have indeed been partially determined, but the results reached, though interesting and valuable, can by no means be considered satisfactory.

We have thus glanced briefly at the present condition of our knowledge of the motions of the principal bodies of the solar system. Only four cases have been found in which we cannot fully explain these motions so far as known, by Newton's law of gravity. The unexplained discordances are the motion of the perihelion of Mercury, and the accelerations of the mean motions of the moon and the two periodic comets just named. But in each case, except perhaps the first, there exists a plausible explanation other than a modification of Newton's law.

If we go beyond the solar system, we cannot tell whether Newton's law does or does not apply without modification to all parts of the universe. It is principally in the hope of answering this question that double star observations are carried on, and in the case of the many binary systems already detected, Newton's law is satisfied within the errors of observation. Nevertheless, this evidence is purely negative, and its value, it seems to me, not at all commensurate with the labor expended upon it, unless it be in the case of such objects as Sirius, whose observations may assist in the solution of the problem of irregular so-called proper motion. The angles subtended are in general so small that relatively large personal errors are unavoidable, so that, even though their motions be controlled by a law or laws of gravity widely different from that of Newton, it is not likely that such differences can be proved with any degree of certainty. It is rather to the study of the proper motions of the fixed stars and of the nebulæ, and then only after a lapse of hundreds, and perhaps thousands of years, that we must look for a solution of this question.

PAPERS READ.

OBLITERATION FROM ILLUMINATION (STELLAR PHOTOMETRY). By HENRY M. PARKHURST, New York, N. Y.

[ABSTRACT.]

IN order to establish the principle that illumination of the field tends to vary the scale of the wedge, I observed Arcturus two hours before sunset with four different apertures differing by half a magnitude. It required about twenty minutes for the approaching twilight to cause the star to become visible with an aperture half a magnitude smaller. It required only four minutes from the first glimpse of the star with each cap for the star to pass entirely across the wedge unextinguished. The effective value of the wedge under that illumination was only about $^{m}.1$, whereas it had been determined to be with a dark sky about $2^{m}.3$. Daylight illumination reduced the effect of the wedge in extinguishing stars to less than one-tenth.

The formulæ showing the effect of illumination upon obliteration, applicable to a dark sky, to moonlight, and to twilight, have been deduced by an indirect process, observing the effect of shades. From these formulæ subsidiary tables have been formed, from which it appears that there is a second difference amounting to $^{m}.2$ or $^{m}.3$, resulting from the illumination of the dark sky with a large aperture and low power. For stars always observed at the same point of a wedge this becomes a systematic error. Although needing further verification, the substantial accuracy of these tables has been confirmed.

The ratio of illumination to obliteration has been approximately ascertained, enabling tables for the application of the second differences to be adjusted to different magnifying powers and different apertures.

From observations at different ages of the moon and at different distances from the moon, including observations of Nova Orionis within 2° from the full moon, I have formed tables of the obliteration by the moonlight. From observations in the twilight, I have also formed tables of obliteration from twilight. But there remains to be formed a table from which the combined effect of the two can be ascertained.

Whenever the stars to be observed are sufficiently bright, I prefer to observe them with my deflecting apparatus (described in Vol. XVIII of the Annals of the Harvard Observatory, in a paper just issued) which is unaffected by illumination. But logarithmic caps cut off four-fifths of the light, so that the wedge will measure stars $1^{m}.7$ fainter. If the necessary corrections are applied, the wedge can be used for stars up to the quadrature of the moon and half an hour before the end of twilight, which could not be seen at all with the logarithmic caps.

A METHOD OF REPRESENTING THE IMAGINARY ELEMENTS OF A GEOMETRIC FIGURE AND OF USING THEM IN CONSTRUCTION. By JAMES MCMAHON, Ithaca, N. Y.

[ABSTRACT.]

1. *Point-graphs.* Call the real points $(a \pm a', b \pm b')$ the *graphs* of the conjugate imaginary points $(a \pm ia', b \pm ib')$. Harmonic properties.

2. *Graph-locus.* If a line meet a conic in two conjugate imaginary points, to find the locus traced by the graphs of these points, as the line moves parallel to itself. To find the real chords joining the imaginary intersections of conics, in certain cases.

3. *Line-graphs.* Call the lines $(l \pm l')\, x + (m \pm m')\, y + (n \pm n') = 0$ the *graphs* of the conjugate imaginary lines $(l \pm il')\, x + (m \pm im')\, y + (n \pm in') = 0$. These two line-pairs have the same angle-bisectors, and are cut by a transversal perpendicular to an angle-bisector in an imaginary point-pair and their real graph-pair.

4. *Orthographs and skew-graphs.* For transversals in a given direction not perpendicular to an angle-bisector, the graph-locus is easily constructed; it is a pair of lines, which may be called the *skew-graphs* for the given transverse direction, to distinguish them from the *orthographs* (for the orthogonal direction). All the graphs for different directions form an involution, of which the imaginary line-pair are the double lines.

5. Given the respective graph-pairs of two imaginary points, not conjugate, to construct (by means of line-graphs) the imaginary line joining them; and reciprocally.

6. Given (by means of graphs) an imaginary line and an imaginary point in it, to construct the imaginary line that passes through the point making a given angle with the first line. Special applications.

ON THE VALUE OF THE SOLAR PARALLAX DEDUCIBLE FROM THE AMERICAN PHOTOGRAPHS OF THE LAST TRANSIT OF VENUS. By Prof. WM. HARKNESS, Washington, D. C.

[ABSTRACT.]

IN this paper an account was given of the instruments and processes employed by the United States Transit of Venus Commission to determine the solar parallax from photographs of the transit of Venus which occurred in December, 1882. Let π be the solar parallax, and δA and δD respectively the corrections to the right ascensions and declinations of Venus given by Hill's tables of that planet. Then, upon the assumption that Hansen's tables of the sun are correct, there resulted from measurements of the distances between the centers of the sun and Venus made upon 1475 photographs, taken respectively at Washington, D. C.; Cedar Keys, Fla.; San Antonio, Tex.; Cerro Roblero, N. M.; Wellington, South

Africa; Santa Cruz, Patagonia; Santiago, Chili; Auckland, New Zealand; Princeton, N. J.; and the Lick Observatory, Cal.;

$$\pi = 8.847'' \pm 0.012''$$
$$\delta A = + 2.893$$
$$\delta D = + 1.254$$

and the corresponding mean distance from the earth to the sun is 92,885,000 miles, with a probable error of only 125,000 miles.

These numbers are doubtless close approximations to the results which will be obtained from the complete discussion of all the photographs, but they cannot be regarded as final for several reasons, chief among which is the fact that the reduction of the position angles of Venus relatively to the sun's center is still unfinished. It is likely that when these angles are combined with the distances the probable error of the parallax will be somewhat reduced.

The photographs taken at the Lick Observatory seem to indicate that for altitudes four thousand feet above the sea the values of the refraction given by the tables in general use are somewhat too large.

THE DIRECTIONAL THEORY OF SCREWS. By Prof. E. W. HYDE, Station D, Cincinnati, Ohio.

DEFLECTIONS OF THE PLUMB-LINE AND VARIATIONS OF GRAVITY IN THE HAWAIIAN ISLANDS. By ERASMUS D. PRESTON, U. S. C. and G. Survey, Washington, D. C.

ON A NEW METHOD OF CONSTRUCTION FOR EQUATORIAL DOMES. By Prof. G. W. HOUGH, Director Dearborn University, Chicago, Ill.

ON A NEW CATALOGUE OF VARIABLE STARS. By SETH C. CHANDLER, Cambridge, Mass.

A NEW SHORT-PERIOD VARIABLE IN AUTLIA. By Prof. HENRY M. PAUL, U. S. Naval Observatory, Washington, D. C.

LAWS OF FREQUENCY OF ERRORS OF INTERPOLATED LOGARITHMS, ETC., DEPENDENT ON FIRST DIFFERENCES; AND A COMPARISON OF THE THEORETICAL WITH THE ACTUAL DISTRIBUTION OF THE ERRORS OF 1000 INTERPOLATED VALUES. By Prof. R. S. WOODWARD, Geological Survey, Washington, D. C.

SOME CONSIDERATIONS ON THE FUNDAMENTAL IDEAS OF QUATERNIONS. By Prof. E. W. HYDE, Station D, Cincinnati, Ohio.

A DESIDERATUM IN THE AMERICAN EPHEMERIS. By Prof. GEORGE C. COMSTOCK, Washburn Observatory, Madison, Wis.

FUSIYAMA, JAPAN, AS A SITE FOR A MOUNTAIN OBSERVATORY. By Prof. DAVID P. TODD, Director Lawrence Observatory, Amherst, Mass.

ON THE SUPPOSED CANALS ON THE SURFACE OF THE PLANET MARS. By Prof. ASAPH HALL, U. S. Naval Observatory, Washington, D. C.

NOTE ON THE MATHEMATICS OF THE SEISMOSCOPE. By CLARENCE A. WALDO, Terre-Haute, Ind.

ON SOME OLD AND NEW THEOREMS IN SOLID GEOMETRY. By H. B. NEWSON, Mt. Gilead, Ohio.

ORBIT OF BROOKS' COMET, 1888 C. By Prof. LEWIS BOSS, Albany, N. Y.

PRELIMINARY ELEMENTS OF THE ORBIT OF COMETS, 1888 IX. By WILLIAM HOOVER, Athens, Ohio.

ON THE MEASURE OF INCLINATION OF TWO PLANES IN SPACE OF FOUR DIMENSIONS. By Prof. IRVING STRINGHAM, University of California, Berkeley, Cal.

ON CENSUS MAPS. By Dr. FRANZ BOAS, New York, N. Y.

SECTION B.

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ADDRESS

BY

PROFESSOR ALBERT A. MICHELSON.

VICE PRESIDENT, SECTION B.

A PLEA FOR LIGHT WAVES.

It is no doubt universally conceded that no era in the world's history has ever seen such immense and rapid strides in the practical applications of science as that in which it is our good fortune to live. Especially true is this of the wonderful achievements in the employment of electricity for almost every imaginable purpose. Hardly a problem suggests itself to the fertile mind of the inventor or investigator without suggesting or demanding the application of electricity to its solution.

Do events in this fast age follow so rapidly that the delays of even the fastest express trains and steamers are unendurable — the remedy is electricity. Is the labor of animals slow and expensive, or the carriage of the motive power itself, as well as its load, dusty, noisy and troublesome — the remedy is electricity. Are the barbarous tallow candle and the almost semi-barbarous use of gas for illumination totally inadequate to bridge over the hours of night — the remedy is electricity.

It would be wearisome to merely mention a tithe of the problems already solved or those on the eve of successful solution — nor is it at all necessary to insist on the richness of the harvest to be gathered by the successful experimenter in this fertile field. Neither is it surprising that so many world-famous men should have de-

voted almost their whole lives to the pursuit of this most fascinating study.

In the enthusiasm aroused by so many wonderful, beautiful and bewildering results, such varied and far-reaching discoveries in the vast fields of this subtile and powerful agent, it is not to be wondered at—or indeed entirely to be regretted—that possibly a great deal of attention has been diverted from the sister department of light. Undoubtedly, there have been many important developments and improvements in optical instruments — the microscope, the telescope, the spectroscope and the camera may be said to have reached the point of practical perfection — and it is equally true that the observations and discoveries made with the help of these have more than justified the high expectations which their advent created. Certainly the wonderful impulse the study of biology has received by the revelations of the microscope; the enormous increase in our knowledge of the size, distance and motions of the heavenly bodies, due chiefly to the little less than marvellous power and precision of our telescopes; the knowledge of solar and stellar physics — which a few years ago would have been thought visionary if not impossible — attained by the spectroscope, now so happily supplemented by the camera; the insight gained into the structure of matter by spectroscopic interpretation of the messages which its molecules impart to the luminiferous ether,—these are all even more truly wonderful and important than any of the astonishing marvels of electricity. But their original conception belongs to an era that is past.

If we except the exquisite results obtained in the manufacture and use of diffraction gratings and the very important work accomplished by the bolometer (a purely electrical invention, by the way), it may well be questioned whether, within the last twenty years, there has been a single epoch-making discovery or invention either in theoretical optics or in its applications.

It may, perhaps, be argued that the department of optics has been fairly completed; that its theory (though still imperfect in many important points) has been fairly well developed and the range of its applications fairly well understood. The unexpected wonders it has already accomplished make it somewhat hazardous to reply that these same observations may *now* be applied to electricity and magnetism. Still it is safe to say that at any rate the more important facts and laws as well as the more promising lines

of the development of their applications, are now fairly well known, and that inducements to their further study and development are not wanting.

If, therefore, physicists would devote a larger share of their careful study to the completion of optical theories and to the application of light as an instrument of measurement and investigation— it need never be feared that there would be a lack of electricians to carry forward to their completion, upon lines already well developed, the principles and facts already known.

It is mainly with a view of attempting to interest brother physicists and investigators in this to me most beautiful and fascinating of all branches of physical inquiry that I venture to present a limited number of problems and I think promising fields for investigation in light, together with some crude and tentative suggestions as to their solution.

The investigations here proposed all depend upon the phenomenon of interference of light waves. In a certain sense all light problems may be included in this category, but those to which I wish to draw your attention are specially those in which a series of light waves has been divided into two pencils which reunite in such a way as to produce the well-known phenomenon of interference fringes.

The apparatus by which this is effected is known by the inconvenient and somewhat inappropriate name of "interferential refractometer." Among the many forms of the apparatus several are fairly well adapted to the work they have already accomplished, but all are open to serious objections. In all the forms which employ a broad luminous source of light, the plane in which the interference fringes are most distinct, is found to vary rapidly with slight changes in adjustment; in fact, it may happen that different portions of the same fringe appear at enormously different distances, so that it is impossible to fix the true position of a fringe or even to count the number which pass a given fixed point. This very serious objection is avoided by using, as the source of light, a narrow illuminated slit, but of course at a sacrifice of light and of convenience and simplicity. Both classes of instruments are open to the objection that the two pencils are very close together, rarely more than a centimeter apart. For some purposes this may be an advantage, but for many purposes it is a serious defect. Finally, none of the forms in general use are adapted to experiments in

which there is a considerable difference in path between the two pencils.

The instrument which I had the honor of describing to the section at the last meeting is free from all the objections mentioned. It is simple in construction; with a little familiarity it is easily adjusted; it may be used with a broad luminous surface; the pencils may be separated as far as desired; and when properly adjusted *the position of the fringes is perfectly definite.*[1] As an additional advantage it may be stated that this is probably the only form of instrument which permits the use of white light (and therefore of the identification of the fringes) without risk of disturbing the position of either surface by contact or close approximation. It is chiefly this property which renders this instrument peculiarly adapted to the comparison of standards of length.

As this form of refractometer has already proved its value in several experiments already completed and in the preliminary work of others now under way, I may be permitted to recall the chief points of its construction and theory. A beam of light falls on the front surface of a plane parallel piece of optical glass at any angle—usually forty-five degrees—part being reflected and part transmitted. The reflected portion is returned by a plane mirror, normal to its path, back through the inclined plate. The second or transmitted portion is also returned by a plane mirror and is in part reflected by the inclined plate, thus coinciding with the transmitted part of the first pencil; and the two pencils are thus brought to "interfere."[2] A little consideration will show that this arrangement is exactly equivalent to an air-film or plate between two plane surfaces. The interference phenomena are therefore the same as for such an air-plate.

If the virtual distance between the plane surfaces is small, white light may be employed and we have then colored fringes like Newton's rings or the colors of a soap-film. If the distance exceeds a few wave lengths, monochromatic light must be employed.

[1] It may incidentally be mentioned that extraneous reflections—such as usually accompany most of the phenomena of interference—may be almost entirely avoided by a transparent film of silver on the front surface of the glass plate where the rays separate; and accordingly the fringes in white light present a purity and gorgeousness of coloration that is only rivaled by the colors of the polariscope.

[2] A second plane parallel plate of the same thickness and inclination is placed (for compensation) in the path of the first pencil.

We may confine our attention to the case of two parallel surfaces. Here it can readily be shown that the fringes are concentric circles, the common axis of the rings being the normal passing through the optical centre of the eye or telescope. Further they are most distinct when the eye or the telescope is focussed for parallel rays. In any other case we are troubled with the same perplexing changes of form and position of the fringes as already noted.

If now one of the mirrors have a motion normal to its surface the interference rings expand or contract; and by counting the fringes as they appear or disappear in the centre, we have a means of laying off any given distance in wave lengths.

Should this work of connecting the arbitrary standard of length — the yard or the metre — with the unalterable length of a light wave prove as feasible as it is hoped, a next step would be to furnish a standard of mass based upon the same unit. It may seem a little like exaggeration to say that the solution of this problem may admit of almost as high a degree of accuracy as the preceding.

Suppose a cube, ten centimetres on a side, with surfaces as nearly plane and parallel as possible. Next suppose a testing instrument made of two parallel pieces of glass, whose inner surfaces are slightly farther apart than an edge of the cube.

The parallelism and the distance of these surfaces can be verified to a twentieth of a wave. Now apply this testing instrument to the three pairs of surfaces of the cube and determine their form, parallelism and distance to the same degree of accuracy. We have thus the means of measuring the volume of a cubic decimeter with an error less than one part in a million.

A very convenient and accurate method of making the determination of the weight of this volume of water at its maximum density has been suggested by Professor Morley, which consists in making the cube hollow, so that it will have almost exactly the same density as the water. On weighing the cube in water the excess of weight may be as small as required and may be most accurately measured by a very light and sensitive balance.

It does not seem extravagant to say that by some such plan as this we may obtain a standard kilogram which will be related to the standard of length with a degree of approximation far exceeding that of the present standard.

In the manufacture of plane surfaces, the only practicable method

of testing their accuracy is to place the surface close to a standard plane and examine the appearance of the Newton's rings formed in the air film between them. This process when executed with proper care is undoubtedly the most accurate, and, indeed, is the only one possible for producing a standard surface; but it is attended with a number of inconveniences, among which may be mentioned the use of sodium light, the troublesome reflection from the first surface, the faintness of the fringes when the surface to be tested is metallic and the difficulty of getting rid of dust between the surfaces. All of these inconveniences are avoided by the use of the refractometer. For this purpose the apparatus is placed in a vertical plane, the lower mirror, which would then be horizontal, is replaced, first, by the test plane and then by the surface to be worked. The interference fringes in white light can then be conveniently studied while the surface is being corrected.

Another application of this apparatus, suggested by Professor Morley, is the measurement of coëfficients of expansion. For this purpose a bar is provided with silvered glass mirrors at each end (both facing the same way) and a second bar of the substance to be examined and of the same length is furnished in the same way. These are placed in the refractometer, so that the front mirrors, as well as the rear ones, give interference fringes in white light. The auxiliary bar is kept at zero. The bar to be examined is heated, and the fringes which pass at the front surface are counted as the bar expands, the fixity of the rear mirror being controlled by the colored fringes at its surface. In this method the bars may be a meter in length, and, therefore, the accuracy of the determination would be proportionally greater than in the celebrated experiments of Fizeau, in which the length was limited to a few millimetres.

Evidently the same disposition will also serve for measurements of coëfficients of elasticity, with the evident advantage of studying the elastic properties of the substance in thick rods or bars instead of small wires. This method of investigation is not limited to the determination of changes in length, but is quite as applicable to changes in density and optical properties; particularly to the effect on the velocity of light in solids, liquids and gases due to alterations in temperature, pressure, or magnetic or electrical conditions.

It may be mentioned that a great deal of valuable work has already been accomplished in this direction. I need only cite the

very interesting and important experiments of Quincke on the compressibility of liquids; of Jamin on the variation of index of refraction of water and of Ketteler and of Mascart on the index of refraction of gases.

It seems somewhat curious that, while the immense advantage of the refractrometer as an accurate means of measuring indices of refraction has been so fully appreciated, its use should be limited to differential measurements. Thus, while it is easy to measure indices of gases, since the difference in optical path for gas and vacuum is so small, the indices of solids and liquids can only be determined in thin plates, and the accuracy of such measurements must be limited to that of the estimation of the thickness. Such experiments may furnish the data for very interesting and important conclusions concerning the index of refraction and specially the anomalous dispersion of intensely opaque substances, such as metals and quasi metallic bodies. In such work the advantage of the interference method over the prismatic must be quite apparent; but I hope to show that for all measurements of refraction and dispersion—for solids and liquids as well as for gases—this method promises results which may far surpass those given by the prism.

Suppose a piece of the substance cut in the form of a plane parallel plate. The accuracy, parallelism and distance of the surfaces in wave lengths may be determined exactly as in the case of the proposed standard cubic decimeter. Next the nearest whole number of waves in the solid can be determined either by actual count or perhaps more conveniently by a method described in a previous paper. The residual fractional parts of a wave may also be found as there described, or by direct observation of the interference rings between the two surfaces.

The measurement of the index of refraction of a liquid is even more simple. A vessel is made with plane parallel sides, and the number of waves between the inner surfaces determined, first, when empty and then when filled with liquid.

The ratio of these two numbers will be the index of refraction. It will be noted that the only observations required in this process are the counting of two numbers; and as Professor Mendenhall has aptly remarked, a mistake in counting of a whole number is not an error but a blunder.

A blunder very easy to make, be it noted, in dealing with such large numbers as two or three hundred thousand, but whose chance

of occurring may be indefinitely diminished by making several independent observations with different kinds of light.

Perhaps one of the most important applications of the method is the determination of the wave length of standard lines, both relative and absolute. In the paper above referred to, it was stated that the maximum difference of path at which interference fringes are visible, had been increased to over two hundred thousand waves (Fizeau's number is 50,000) by using light from highly rarefied sodium vapor in an exhausted tube. Since then I have observed interference under similar conditions with thallium with a difference of over three hundred and seventy thousand waves, and with mercury, with a difference of more than five hundred and forty thousand waves.

By repeated measurements of the diameters of the interference rings, the fractions of a wave can be found to within a fiftieth—which means that the number of waves in this fixed distance can be found to within less than one part in twenty-five million. Any two kinds of light of this degree of purity can be compared with this same precision. The accuracy of the measurement of absolute wave-lengths will of course depend on the accuracy with which the fixed distance can be compared with the standard meter; and this may be estimated as one part in two million.

The results of the remarkable work of Rowland do not claim a much greater degree of accuracy than one part in half a million for relative determinations; while the elaborate research of Bell on absolute wave-lengths claims but one in two hundred thousand.

We have thus at any rate a very promising method of excelling by far the best results that can possibly be obtained by the most perfect gratings.

It may possibly help to realize the very considerable superiority of this instrument over the grating—at any rate for the class of work in question—if I recall to your attention the fact that by its means it has been possible to show that the red line of hydrogen is a very close double. A short time ago the same was found true of the green thallium line. Both these lines are something like a fiftieth of the distance of the sodium lines, and like these are of unequal intensity. It is even possible to measure this very small interval easily to within a fourth of one per cent. Following are the numbers obtained for the distance from one maximum or minimum of distinctness to the next:—

	Maxima	Minima
	1.025	1.012
	1.050	1.012
	1.025	1.050
	1.017	1.033
	1.000	1.025
	1.038	1.000
	1.021	1.017
Mean	1.025	1.021

One unit means a distance of 24.6 mm. which gives for the average distance 25.2 mm. and for the ratio of the wave-lengths of the two lines 1.0000212.

Closely connected with the preceding investigations is the study of the effect of the temperature, thickness, and density of the source on the composition of the radiations, as shown by the symmetrical or unsymmetrical broadening of the spectral lines and the consequent shifting of their mean position. This question has quite recently been taken up by H. Ebert and the results he has already obtained are very promising. The principal effects noted are: first, the shortening of the difference of path at which interference can be observed; secondly, the shifting of the fringes as the mean wave length changes. Ebert has shown that the interference method is far more delicate than the spectroscopic; and by its means he has established two conclusions which, if verified, are of the greatest importance—namely; first, that the chief factor in the broadening of the spectral lines is the increase in density of the radiating body; secondly, that the broadening, in all the cases examined is unsymmetrical—causing a displacement of the line toward the red end of the spectrum. The importance of these conclusions, in their relation to the proper motions of the heavenly bodies and their physical condition, can hardly be overestimated. The value of results of this kind would, however, be much enhanced if it were possible to find a quantitative relation between the density of the radiating substance and the nature of its radiations. In the case of hydrogen enclosed in a vacuum tube this could readily be accomplished. It may, however, be objected that it would be difficult in this case to separate the effects of increased density from those due to the consequent increase in the temperature of the spark. The problem of the temperature of the electric discharge in rarefied gases is one which has not yet been solved. In fact it may seriously be questioned whether in this case temperature has anything to do with the accompanying phenomena of light; and it

appears to me much more reasonable to suppose that the vibratory motion of the molecules is not produced by collisions at all but rather by the sudden release of tension in the surrounding ether.

Whether true or not, the results obtained and interpreted by this hypothesis would be of great interest. The method could be applied directly to any substance, mercury for instance, for which the relation between the temperature and the pressure is known. For substances for which this relation has not been established, as sodium, thallium, etc., the density may be found by heating the substance in a tube closed with plane parallel glass ends and measuring its index of refraction. The density will be very approximately proportional to the excess of this index over unity.

Aside from its application to this problem, it seems highly probable that this method of measuring the density and pressure of vapors may be made to yield excellent results in cases where these are far too small to be measured directly.

It may not be entirely out of place in this connection to present a few observations concerning the causes of the broadening of the spectral lines. It seems to me that by a thorough and systematic study and discussion of this phenomenon we have a possible means of materially increasing our knowledge of a subject, of which we are at present in almost total ignorance: namely, the real action of the forces and motions of vibrating atoms and of the ether which transmits these vibrations in the form of light.

The possible causes of the broadening of spectral lines are as follows:—

First, the addition of vibrations of periods differing from the normal period, due to the influence of neighboring molecules; secondly, the effect on the wave length due to the velocity of the molecules.

It is evident on considering the second cause, that it could not possibly account for more than a small fraction of the effects observed. For example, to effect a change in wave-length corresponding to the difference between the two sodium lines, would require velocities of the order of three hundred thousand meters per second, over a hundred times as great as the velocities given by the kinetic theory. But it is also clear that when a gas is so rarefied that the first cause cannot possibly produce any perceptible effect, the second cause would be quite sufficient to limit the fineness of the lines, and to impose a definite limit to the difference of path at which interference is visible; and it is worthy of note

that the actual limits observed are of the same order of magnitude as those given by the kinetic theory.

There is still a third cause which might limit this distance, but which would not have any effect in broadening the lines; namely, the diminution in the amplitude of the vibrations after collision. There must be such a diminution and it would evidently be the more marked the more rapidly the energy was transferred to the ether, that is, the brighter the light. If the effects due to this cause alone could be separated from the others it would be possible to measure the diminution in amplitude and therefore the rate of transference of the energy. Thus it may be shown that a vibrating sodium atom gives up to the surrounding ether less than six millionths of its energy at every oscillation.

Returning to the first and chief cause of broadening, it may be remarked that the universal opinion of scientific men seems to be that during collisions between the molecules the vibrations are entirely "irregular;" and the longer the collisions last in proportion to the time between collisions, the more intense will be the light due to these "irregular" vibrations, and consequently the broader the lines and the more impure the light.

The following consideration would seem to show that this explanation will not hold.

If, in the refractometer, so frequently referred to, white light be used, all phenomena of interference are lost to sight when the difference of path exceeds a few wave lengths, for the well-known reason that the fringes due to the infinite number of different kinds of light are superposed, thus producing a uniform illumination. If now this light be analyzed by a spectroscope, the spectrum will be traversed by well-marked interference fringes which are the finer and closer, the greater the difference of path of the interfering pencils. Now, I have observed such interference fringes in the white light from the incandescent carbons of an arc light when this difference amounted to thirty thousand waves. And it may be added that this limit was reached by the closeness of the lines rather than by their indistinctness.

It seems to me that the only conclusion which can be drawn from this experiment is that in the light from an incandescent solid the vibrations must be *isochronous* for at least thirty thousand waves. The same observation applies also to the so-called "irregular" vibrations of the broadened sodium lines, for the same limit (about thirty thousand waves was also observed in this case). The

inference seems irresistible that the broadening is not caused by "irregular" vibrations, but by the addition of vibrations whose intensity is greater the nearer their period is to that of the normal vibrations and which may be almost if not quite as regular as the normal vibrations themselves.

If these conclusions be granted we must profoundly modify our conception of radiation in solids and liquids, or at least that part of it which supposes that such radiation produces a continuous spectrum because the molecules have no "free path," and, therefore, no proper periodic vibrations.

What, then, is the nature of the effect produced by the collision of molecules? If it be to produce or reinforce vibrations differing from the normal type, it must be granted that these new vibrations are *isochronous*. If so, they must be due either to a change in the form or in the mass of the molecule itself produced by collision, such changes tending to revert back to the type when the frequency of the collisions is not too great. The only alternative is to suppose that the molecules differ among themselves, either in form or weight. In this case, the molecules agreeing most nearly with the type and hence having a proper period differing but little from the normal would be more easily set in vibration than the others, or their vibrations once started would outlast the others. Accordingly, when a gas is very much rarefied, the collisions are few, hence only the typical vibrations persist; but when the collisions are frequent the other vibrations are also sustained.

I fear I have wandered so far from the subject of this address, if such a name be at all appropriate, ever to return; and, though many other interesting and important applications of light-waves clamor for recognition, I fear they would be wearisome even to enumerate.

I fear also that it may with some justice be said that I have made a plea for my own instruments and theories, rather than "a plea for light waves;" and still more that I have presented many crude and half digested ideas, when it would have been more to the purpose to present facts and results of diligent study and careful experiment.

In extenuation and in conclusion I can only hope that if I have created the slightest interest in the matters here presented for your consideration, if there be any chance that even a few of the seeds may germinate, grow, blossom and bring forth fruit worthy of acceptance, my purpose will be accomplished.

PAPERS READ.

ON THE DIFFUSION OF HEAT IN HOMOGENEOUS RECTANGULAR MASSES, WITH SPECIAL REFERENCE TO BARS USED AS STANDARDS OF LENGTH. By R. S. WOODWARD, U. S. Geological Survey, Washington, D. C.

[ABSTRACT.[1]]

THIS paper discusses the laws of diffusion of heat in rectangular masses of any dimensions, and aims to give to the various problems that arise, solutions which may be readily used in computing numerical results.

The assumptions on which the work is based are the following: (1) that the mass has initially a uniform temperature; (2) that it cools or heats in a medium of sensibly constant temperature; (3) that the exterior and interior conductivity and thermal capacity of the mass remain constant.

Starting from Fourier's solution of the general problem defined above, the obstacles met in applying that solution are pointed out. Fourier's method requires a certain number of the roots of the three transcendental equations which express the boundary conditions of the mass. The new solution either avoids the difficulty of determining those roots altogether, or makes use of the first root only of each equation. Incidentally, however, methods of computing the roots are given.

The pervading idea of the investigation is this, viz.: to separate the terms independent of from those dependent on the exterior conductivity, or emissivity. In accordance with this idea, the problem divides itself into two cases, in the first of which the ratio formed by the product of the emissivity and a linear dimension of the mass is less than unity, and in the second of which that ratio is greater than unity.

Formulas for certain average temperatures of special interest relative to standards of length are given, viz.: the average temperature of the whole mass, of any face and of the axis of a bar.

Special attention is given to the needs of the computer in the derivation and arrangement of formulas, and the application of nearly every formula is illustrated by a numerical example.

[1] The paper will be published in full in Annals of Mathematics, Vol. 4, No. 4.

A NOTE UPON RETINAL PHOTOGRAPHY. By CHARLES A. OLIVER, M.D., 1507 Locust St., Philadelphia, Pa.

[ABSTRACT.]

IN a desire to place upon record the author's progress in this direction, he would state that at the present writing (July, 1888) he, in conjunction with Dr. Wharton Sinkler, has obtained a fixed and easily adjusted apparatus consisting of an ophthalmoscopic attachment to a small field camera that can be adjusted to any position, a boxed oil light so arranged that the emergent rays pass through a two inch focus biconvex lens before becoming impinged upon the ophthalmoscopic mirror, a small fixed head-rest for the subject, and a spectacle frame lens attachment to be placed before the subject's eye. The experiments, which were at first conducted with the artificial eye of Perrin and which resulted in negatives with clearly cut discs of about twenty-five millimetres in their long diameter, surrounded by an area of one hundred millimetres upon which the retinal vessels and gross changes in the retina and choroid could be plainly seen for nearly seventy-five millimetres area around the disc, the whole being entirely devoid of any disturbing light reflexes (these were untouched and unremagnified), have now been transferred to studies with the living human eye. Here the only difficulties have been the interfering corneal reflex and the heat experienced. The latter has been successfully combatted by resource to Dr. Howe's alum bath, while the former is the part of the subject now under experiment. The entire method with a full description of the completed apparatus will be published as soon as obtained.

THE QUALITY OF MUSICAL SOUNDS. By W. LECONTE STEVENS, Brooklyn, N. Y.

[ABSTRACT.]

IN this paper a brief sketch is given of the method adopted by Helmholtz in his investigation on musical quality, which resulted in the conclusion that "differences in musical quality of tone depend solely on the presence and strength of partial tones, and in no respect on the differences of phase under which these partial tones enter into composition."

A résumé is given of a paper on the "Beats of Imperfect Harmonies," read by Sir William Thomson in 1878 before the Royal Society of Edinburgh, in which he expresses conclusions inconsistent with those previously reached by Helmholtz.

A description is then given of the wave siren invented by Rudolph Koenig of Paris, for the purpose of testing the effect of change of phase on quality of tone. This instrument was brought to America a few years ago,

but was injured in transit so that it could not be operated. It has since been further improved. The writer has had an opportunity to test its action in company with M. Koenig, and believes that through this instrument the truth has been established, that variation in phase among the components of a compound sound is a distinct element in determining musical quality. Helmholtz's view is that generally found in text-books.

On dynamical units. By Prof. T. C. Mendenhall, President of Rose Polytechnic Institute, Terre Haute, Ind.

[Abstract.]

Confusion exists in the use of the terms *weight* and *mass*, much of which arises out of the use of the word *pound* in two senses. It is generally and properly used both as a unit of mass and as a unit of force. This fact should be clearly recognized in text-books of engineering and it is desirable that the pound as a unit of force should be accurately defined. The definition suggested is that it is the force of attraction between the unit of mass called a pound and the earth. Dynamical equations should be freed from "g" (gravity), the units being poundals, if force, or foot-poundals if work. These can afterward be units reduced to the more common gravitation units if desired.

Protection of watches against magnetism. By C. J. H. Woodbury, Boston, Mass.

[Abstract.]

The growing and even general use of electricity for illumination and for the transmission of power has added much to the knowledge of electrical phenomena, and also called the general attention of the public to matters long known to those familiar with physical science.

Public attention was first called to the influence of magnetism on watches at the Paris Electrical Exposition, where visitors could leave their watches near the entrance.

My first experience with the effects of electricity on a watch were decidedly unpleasant; for I was struck by lightning in July, 1881, at a time when I was thoroughly drenched with water. The only permanent damage was received by my watch, which was marked on the back of the case with two straight black stripes, each about half an inch in width, extending across the back and joined together at one end like a letter V. Three of the arbors in the movement were broken, but there was no discoloration except on the outside.

Another watch, which possessed a remarkably uniform rate and was highly valued for its accuracy, yielded to the temptation offered by the

magnetic fields around dynamos and became very uncertain, losing time at irregular intervals and at varying rates.

The usual methods of demagnetization were all tried with the result of lessening the evil, but not removing it, as it seemed to be impossible to bring back the movement to the old rate.

It is surprising how weak a magnetic field will stop a watch, a small magnet being sufficient for the purpose; and far weaker magnetic fields will interfere with the rate of a watch to an extent which will injure its value as a timepiece.

There are two types of attachments designed to protect watches against magnetism; one being a small disc of iron taking the place of the inner cover of a watch case; and the other a box or case surrounding the watch case, like the outer case of a bull's eye watch. Without endorsing any of the extreme statements declared to be fundamental principles of magnetism by those engaged in the sale of either of these devices, it is evident that any magnetic body will distort lines of force in a magnetic field; and it was considered worth while first to submit the metal case to direct experiment.

The magnet used was a small dynamo containing cast iron to the weight of about four hundred and fifty pounds. The armature was disconnected from the apparatus and the field coils connected to an electric circuit, in order that the watches should not be subjected to jar from a revolving armature.

A circular piece of cardboard was placed in the case and dusted over with iron filings which had not been magnetized, and the box was closed and placed on a block of wood a foot from one of the poles of the magnet. An electric current sufficient to saturate the magnet passed through the field coils for a few moments, and the circuit was broken before the case was opened in order that the contents should not be directly exposed to any attraction except what was transmitted through the iron forming the case. On opening the case it was seen that the iron filings had been drawn towards one side of the card and arranged in striæ in the ordinary manner whenever they are exposed to a magnetic field.

A watch which had been set to exact time with one in an extreme part of the room was placed in the case, and in a like manner exposed to the magnetic field for five minutes. When the case was opened it was found that the watch had stopped at the instant that the magnet was charged; and the movement has been magnetized to an extent which has robbed it of any present value as an accurate timepiece.

As it was evident that the case did not adequately protect the watch, experiments were not made with the shield referred to, which contained a small fraction, perhaps one-tenth, as much metal as the case.

These watches were not exposed to any more severe tests of magnetism than would be liable to be offered to a watch worn on the person of one around a dynamo or motor, except that a passenger in a motor car would naturally be in a sitting position and the watch would be exposed for a longer time to any lines of force than would be the case around a dynamo,

where the person would not probably remain in a stationary position so long a time.

Some time ago I examined a watch containing the inventions of C. A. Paillard, of Geneva, in which the hair spring and balance were made of alloys of palladium, the escapement being made of steel, and found that although a weak magnetic field did not produce any apparent effect upon the movement, yet it was stopped by exposure to a very strong attraction.

This difficulty would be intermittent in its nature even under the extreme conditions of magnetic force which stopped the watch, because the balance and hair spring were composed of non-magnetic alloys.

Subsequently, when the same make of watches were produced containing the whole escapement as well as balance and hair spring composed of alloys of palladium, I exposed one to the tests mentioned above without stopping it or interfering with its rate.

It is probable that long continued exposure to an intense magnetic field would tend to accelerate the rate of such a watch, because the resistance to cutting the lines of force would slightly reduce the length of the arc of vibration and therefore increase the number of beats during a given time; but I have not had time to learn by experiment whether these conditions of continuous exposure to an intense magnetic field could under any circumstances affect the rate of such a watch. It is evident, however, that a watch worn on the person would not be exposed for a sufficient length of time to receive any modification of rate. In any event, such influence upon rate would not be permanent, but would continue only while the rapidly moving parts were cutting lines of force in a magnetic field.

The alloys of palladium referred to possess an elasticity comparable to steel, as is shown by their practical operations in watches and chronometers. It is undoubtedly not oxidizable in moist air; and the alloys, being composed of both paramagnetic and diamagnetic metals, must approximate more or less closely to a neutral substance.

Several of the types of watches made in this country and in Europe have contained non-magnetic movements, using alloys of other metals than palladium.

The present extensive use of dynamos, and the prospective general introduction of motors, especially upon street railways, will render watches which are unaffected by magnetism a general necessity to all desiring accuracy in their watches.

SOME PHOTOGRAPHIC EXPERIMENTS ON THE COLOR OF THE SKY. By Prof. FRANK P. WHITMAN, Adelbert College, Cleveland, Ohio.

[ABSTRACT.]

LORD RAYLEIGH, in discussion of Tyndall's theory of the color of the sky, showed that the scattering of light from small particles is sufficient to explain most of the phenomena connected with skylight, especially its color and polarization. His experiments on the subject seemed to substantiate the theory.

In 1879 O. E. Meyer found that diffused daylight, which is practically skylight, showed in comparison with direct sunlight a preponderance not of the shorter, but of the longer wave-lengths, making daylight reddish relatively to sunlight.

Prof. E. L. Nichols, in 1885, found skylight to differ on the whole from white light less than that reflected from most white substances, so called. He made the hypothesis that the blue of the sky is mainly subjective, due to the well-known sensitiveness of the eye to short wave-lengths under faint illumination.

The author has attempted to bring these conflicting results to a definite, though qualitative test, by means of photography, thus avoiding complications arising from peculiarities in the eye.

The ordinary gelatino-bromide plate is chiefly sensitive to the shorter wave-lengths. If it be bathed in a solution of erythrosine and exposed in the camera behind a yellow screen, the greatest sensitiveness will be found, not in the blue, but in the yellow.

Suppose two objects, one blue, the other white, but of such brightness that both send the same amount of blue light to the camera. If they are photographed in the same field, the images will appear on the blue-sensitive plate of equal intensity, while on the yellow-sensitive plate the white object will make the stronger impression.

Various white objects illuminated by the sun were photographed against a blue sky on these two kinds of plates. More than thirty photographs were taken of the moon in full daylight. Six were made from a block of magnesium carbonate. White clouds were photographed in a blue sky and landscapes photographed in which a blue haze obscured the distance.

All these experiments led to similar results. The object photographed on the blue-sensitive plate was faint, or hardly distinguishable from the background of the sky, while on the yellow-sensitive plate the image was clear and strong.

The results seem to prove conclusively that the light from the sky is distinctly blue, and, as far as they go, to uphold the theory and experiments of Tyndall and Rayleigh.

ON A METHOD OF DETERMINING THE EMISSIVITY OF A METALLIC BAR COOLING OR HEATING IN AIR. By R. S. WOODWARD, Geological Survey, Washington, D. C.

[ABSTRACT.]

THIS paper shows how from observations of the varying temperature or length of a bar, cooling or heating from an initial uniform temperature in air of constant temperature, the emissivity or surface conductivity may be found in terms of the thermal capacity of the bar. The theory of the process is derived from the last section of the author's paper "On the Diffusion of Heat in Homogeneous Rectangular Masses." The results found by applying the process to certain experimental data are stated.

ON AN EXPERIMENT BEARING UPON THE QUESTION OF THE DIRECTION AND VELOCITY OF THE ELECTRIC CURRENT. By Dr. E. L. NICHOLS of Ithaca, N. Y., and WILLIAM S. FRANKLIN of Lawrence, Kansas.

[ABSTRACT.]

A COIL of wire of 390 turns was driven at a very high rate of speed, the axis of the coil being the axis of rotation. When the coil reached 380 revolutions per second, the linear velocity of the wire in the direction of its own length amounted to 8,000 centimeters per second. By means of two brush contacts at the axis a current was sent through the coil while the latter was in motion. The magnetic moment of the coil was determined by means of a very sensitive astatic pair of magnets carrying a mirror.

Readings were taken with the coil at rest and in revolution, the motion of the coil and the direction of the current being repeatedly reversed. If the electric current result from the flow of a fluid through the wire, in other words, if it may be considered as possessing direction and finite velocity, a motion of the conductor, with or against the current, should produce an appreciable influence upon the deflection of a magnet needle, even though the velocity of the current were very large as compared with that of the conductor. In order to render the detection of this presumably very small effect less difficult, the direct influence of the coil was eliminated by differential winding. Under these circumstances, when the coil was carrying as large a current as it could be made to do without injurious heating, the rotation of the coil was found to be without appreciable effect upon the magnetic moment of the same.

The best results were obtained by sending 4.6 amperes of alternating current of 40,000 alternations per minute through the coil. At a velocity of the wire equal to 8,000 centimeters per second the rotation of the coil produced no effect upon the needle amounting to 0.2 millimeters deflection. The figure of merit of the coil and needle was determined by substituting a coil of continuous winding, its position with respect to the needle being the same as that of the rotating coil, and determining the current necessary to produce 1 centimeter deflection. The sensitiveness of the apparatus was found to be such that a current having direction and a velocity of 1,000,000,000 meters per second would have shown a change in its action upon the needle (when the motion of the coil was 380 revolutions, 8,000 centimeters per second) amounting to 0.1 centimeter deflection, an effect which could not have escaped observation. It follows from the above negative results that if the electric current consists in the flow of a medium or fluid through the conductor, the velocity of the same must be greater than the exceedingly high rate just mentioned. Foepl, who in some recent experiments (Wiedemann's Annalen, 1886), used an apparatus in most essential particulars similar to our own, but one by means of which only relatively very low velocities could have been detected, has reached the same negative conclusion.

On the effect of the added term of the equation of the quadrant electrometer on its deflection curves. By Prof. T. C. Mendenhall, President of Rose Polytechnic Institute, Terre Haute. Indiana.

[ABSTRACT.]

To the well known expression for the moment of the couple acting on the needle of the quadrant electrometer, M. Gouy has added a term depending on the difference of potential between the two quadrants and proportional to the square of this difference. The effect of this term on the deflection curves of the electrometer, as mounted according to Mascart, Thomson or Joubert, is investigated and found to be essentially as follows:—The Mascart equation still remains that of a straight line; the Thomson method reduces the equation to one of the third degree, no longer representing a parabola, but a curve with two branches, each having a point of inflection and an asymptote common to both. It is also shown that the first part of this may be assumed to be a straight line without sensible error, provided the potential of the needle is very large compared with that of the quadrants; thus, if the needle is charged to 5,000 volts, potentials below 100 volts may be compared by this method with less than one per cent error. . . . By the Joubert method of mounting, in which the needle and one quadrant are connected and the other is put to earth, the equation, which without the added term represents a common parabola with the origin at its vertex, is that of a curve somewhat similar to a parabola but with a point of inflection in each branch and an asymptote common to both branches.

Incandescent lamps considered as machines for transforming electrical energy into the energy of light. By Ernest Merritt, Ithaca, N. Y.

[ABSTRACT.]

The author determined the ratio of the energy of the visible rays to the total energy of the lamp. This was done by cutting off the dark heat by a cell of about one decimeter thick, containing a solution of alum, and allowing the rays transmitted to fall on a delicate thermopile. The deflection of the galvanometer in circuit with the pile was then proportional to the energy of the light from the lamp. The alum cell was then removed, and the deflection corresponding to total radiation observed. The ratio of the two deflections gave the efficiency of the lamp at that candle power, when considered as a machine for producing luminous rays.

This ratio was determined in one case by placing the lamp to be tested in a large glass calorimeter and measuring the heat given to the water and the total electrical energy supplied. In both cases corrections were applied, due to the fact that water or alum does not absorb quite all of the

dark heat. In the case of alum this transmission amounts, in extreme cases, to 0.5 per cent. With water it is about 1.5 per cent. A correction was also made for the absorption of light by the water.

The results show that the efficiency of a lamp as a machine does not correspond at all with its commercial efficiency.

One lamp may have a larger "luminous efficiency" than another, and yet require a much larger expenditure of energy to bring it to the same candle power. For example, two Edison lamps were tested: one being quite new, the other old and commercially inefficient. The first gave 16 C. P. at 85 Watts, with a "luminous efficiency" of 4.0 per cent; the second gave 16 C. P. at 100 Watts, with an efficiency of 7.4 per cent.

The values of the "luminous efficiency" of five different lamps at different candle powers are given below in per cents:

	1 C. P.	2 C. P.	4 C. P.	8 C. P.	16 C. P.
Edison, 108 volt, new,	0.6	0.7	1.1	2.2	4.0
Edison, 100 volt, old,	2.3	3.2	4.7	6.3	7.4
Weston, 110 volt,	2.9	3.5	4.1	5.7	
Weston, 70 volt,	2.4	2.9	3.6	4.8	
Bernstein, 20 volt, 8 C. P.,	1.6	2.5	3.7		

The energy per candle power was also calculated from these results. This energy, or the mechanical equivalent of one candle power, decreases as the candle power rises.

The following table gives the values of this mechanical equivalent in Watts:

	1 C. P.	2 C. P.	4 C. P.	8 C. P.	16 C. P.
Edison, 108 volt, new,	0.58	0.30	0.17	0.17	0.20
Edison, 100 volt, old,	.85	.80	.73	.64	.47
Weston, 110 volt,	.84	.68	.51	.37	
Weston, 70 volt,	.75	.57	.42	.37	
Bernstein, 20 volt, 8 C. P.,	.61	.54	.51		

FLOATING DYNAMOMETER. By Prof. J. BURKITT WEBB, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

AT last year's meeting a new form of dynamometer was described with the aid of an illustrative model, which was more however in the form of a laboratory experiment than in that of practical application. During the past year two practical dynamometers have been constructed and have worked satisfactorily in all respects, so that there would seem to be no better method of testing dynamos and motors, especially those of great weight or unsymmetrical form. Especial forms have been devised for other purposes among which is a floating transmission dynamometer capable of giving exact results for scientific purposes.

The photographs show an eight-hundred pound dynamometer mounted with a "Westinghouse Exciter" of nearly that weight.

The "Ark," or "Floating Dynamometer," is applicable to all cases where a "Bracket Cradle" might be used and possesses some advantages which the latter has not.

"OVERHAULING" IN A MECHANICAL POWER. By Prof. J. BURKITT WEBB, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

PROFESSOR BALL, in his book on "Experimental Mechanics," has attempted to state a general law for such "overhauling." His statement is that whenever rather more than half of applied energy is lost in friction the apparatus will not overhaul.

It has been shown by others that this statement is not always correct[1], though the entire fallacy of it may not have been perceived, the law, as we have worked it out, not having such a simple expression.

We will simply show here how a mechanical power may be constructed which will not overhaul and yet lose much less than half the applied energy in friction.

For example let the "power" be a simple lever with the power pulling upward and the weight consequently applied between the power and the fulcrum. Let the weight be applied at one inch and the power at two inches from the fulcrum and let the latter be a journal having a radius of five inches turning in a suitable box or bearing with a friction of one-fifth of the weight; *i. e.*, with a coëfficient of friction equal to the tangent of the angle whose sine is one-fifth. Such a mechanical power will not overhaul and will waste in friction but one-quarter of the energy applied.

This may suggest to any one having the necessary mechanical insight how to construct other non-overhauling mechanisms with other fractions of energy lost in friction.

IMPACT IN THE INJECTOR. By Prof. J. BURKITT WEBB, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

SOME comparisons have been made between pumps and injectors as to the mechanical work performed by them respectively in feeding a boiler; it is thus brought out very forcibly that while the injector wastes no energy it does very little work, most of the steam being used in warming the water. It will be interesting to notice the reason for this because it depends upon the simple principles of mechanics, which though embodied in the usual equations for the injector may not, in that form, be sufficiently appreciated.

[1] Dr. Coleman Sellers, of Stevens Institute, Hoboken, N. J., showed this in a lecture to the upper classes so far as applied to an apparatus consisting of a number of parts.

There are three places in the injector to which we will call attention in examining the action of a particle of steam; (*a*) in the boiler, (*b*) in the vacuum chamber, (*c*) in the mixed column of water and steam. At *a* the steam is practically in a state of rest, but possesses energy capable of doing work. In passing from *a* to *b* work is done in giving to the steam a high velocity, so that at *b* it possesses a large portion of kinetic energy by virtue of which it strikes the water a blow and sets it in motion. Suppose now that the steam, having a velocity v, strikes, say, fifteen times its mass of water and sets it in motion, the combined mass will have a velocity of only one-sixteenth of v, because the momentum before impact, $v \times 1$, must be equal to that after impact, $v \div 16 \times (1 + 15)$.

The kinetic energy possessed by the steam before impact is equal to $\frac{1}{2} \times v^2$ but afterward there will be no more then $\frac{1 + 15}{2} \times \frac{v^2}{16^2}$, so that at impact fifteen-sixteenths of the kinetic energy of the steam disappears, being retransformed into heat by the concussion, it will thus be seen that the injector does primarily transform a large amount of heat into work, but that it uses this work in so uneconomical a way, in forcing the water into the boiler by impact, that most of it is wasted as mechanical work, though saved as heat.

NOVEL FORM OF ELECTRO-MAGNETIC TELEPHONE. By Prof. R. B. FULTON, University, Mississippi.

[ABSTRACT.]

THE author has devised a form of electro-magnetic telephone, which may be used as a receiver or transmitter, in which the lines of magnetic force, the direction of the induced currents, and the direction of the movement of the vibrating parts are each at right angles to the other two. This end is attained by dispensing with the ordinary spool electro-magnet and circular diaphragm.

A tube of thin sheet iron, open at one or both ends, having a polygonal cross section, is magnetized, and has ordinary fine insulated telpehone wire coiled around the outside, the coils being each in a plane perpendicular to the length of the tube. One end of the tube is made a north, and the other a south pole. The coils are placed over those parts of the walls of the tube which are thrown into vibration when the tube is spoken into. The best results seem to be obtained with the above instrument as a transmitter, and the ordinary Bell receiver.

A NEW PRESSURE INDICATOR OR RECORDER (EXPERIMENTAL MODEL). By Prof. W. H. BRISTOL, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

IN this instrument the novelty consists in employing a special form of the well-known Bourdon spring tube in combination with a principle involved in the construction of a pressure indicator which was invented and

patented by Jno. Matthews in 1855. With such a combination it was found to be possible to produce a pressure indicator in which the movement of the registering pointer is positive and of such range for given variations that the usual or all multiplying devices may be dispensed with. The invention of Matthews consisted in making a tube of copper by electric deposition, of circular cross section with one side corrugated. One end being closed, an internal pressure produced an elongation of the corrugated side which, being restrained by the straight side resulted in a bending of the tube, the amount of bending depending upon the pressure. The deflections produced as described are then multiplied to suit the range of reading required.

In the self-registering instrument exhibited the tube has a flattened cross section, is closed at one end, and bent into an approximately sinusoidal form. At several points along its length is secured a flexible strip of same metal as tube or of one having the same coëfficient of expansion.

The bent tube may be considered as a series of Bourdon springs placed end to end. If the bends are of equal radii of curvature, an internal pressure would produce a tendency to straighten each, or collectively to elongate the whole. This elongation is restrained and converted into a magnified lateral motion by means of the flexible strip. If the bends on one side of the tube are of greater radius than those on the other, the motion due an internal pressure will be the resultant of an elongation and a lateral deflection, the lateral deflection being due to the difference in the sums of the forces tending to straighten the bends on the opposite sides of the tube. When the flexible strip is applied to the side of the tube with the larger bends restraining the component of elongation, an increased side deflection is produced.

By mounting such a tube with a marking point attached in connection with a uniformly moving chart, an extremely simple and reliable recording pressure indicator may be devised.

The sensitiveness of the tube may be varied by changing proportions and making different combinations of cross sections, thickness of walls, radii of curvature of bends, number of bends, and length of tube. The same form of instrument is adapted for use as vacuum as well as for pressure indicators.

A NEW INDICATING OR SELF-REGISTERING THERMOMETER (EXPERIMENTAL MODEL). By Prof. W. H. BRISTOL, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE pressure indicator described in the preceding paper is filled with an expansible liquid, as alcohol, and permanently sealed.

Variations in temperature produce expansion of inclosed liquid causing internal pressure, which in turn gives deflections corresponding.

With marking point and chart for recording deflections, a simple self-registering thermometer may be constructed.

A NEW INDICATING OR SELF-REGISTERING BAROMETER (EXPERIMENTAL MODEL). By Prof. W. H. BRISTOL, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THIS instrument is an application of the pressure indicator already described. A tube, made by electric deposition, is exhausted of air and sealed. The walls of the tube being sufficiently light, it is sensitive to small changes of external pressure: as, for instance, atmospheric changes.

For a self-registering instrument it would only be necessary to mount such a tube, with its marking point, in conjunction with a moving chart for receiving the record.

BEST METHODS OF MAKING INSTANTANEOUS PHOTOGRAPHS DURING BOTH DAY AND NIGHT. ILLUSTRATED BY EXPERIMENTS AND PROJECTIONS. By Dr. E. P. HOWLAND, Washington, D. C.

[ABSTRACT.[1]]

INSTANTANEOUS photography by the discovery and introduction of dry plates has given a great impetus to science in every department. It is now necessary that investigators in most branches of science should have a practical knowledge of the art. It records his discoveries by a process of Nature's engraving that is absolute truth. Any advancement in this art by discovery or application interests all investigators. In a condensed form, I will state some of the most important facts in relation to this subject and exhibit specimens of actual results.

The best lens for taking instantaneous photographs of landscapes and moving objects at a distance of one hundred feet and over is a single achromatic lens with the largest diaphragm that will give good definition, a single achromatic obstructing less light than a double achromatic lens with the same sized diaphragm and equal focus. As a single achromatic cannot be used for near objects, on account of distortion of the image, a double achromatic lens is absolutely necessary with the largest diaphragm that gives good definition. Magnesium in powder with any substance that will cause a rapid combustion is the best substance known for producing a light for instantaneous photography in dark rooms or at night. Chlorate of potash, sulphur, guncotton, sulphide of antimony, picric acid and other substances can be used with the magnesium; but I prefer magnesium with one-third its weight of flour of sulphur. This has given me satisfaction, is a safe compound to use and can be ignited by any burning substance or the electric spark. With the camera six feet from the object and the flash light two feet back and a little to one side of the camera, thirty grains of magnesium and ten grains of sulphur with a reflector behind the flash, will give a good photograph on an ordinary plate of No. 25

[1] This paper was accompanied by an exhibition on the screen of the photographs alluded to.

sensitometer or an orthochromatic plate No. 27. At twelve feet distant, one hundred twenty grains of magnesium and forty grains of sulphur are required. Good photographs can be taken on a 25 sensitometer plate at the distance of twenty-four feet by flashing four hundred and eighty grains of magnesium and one hundred and sixty grains of sulphur. On a 35 sensitometer plate one-half of this quantity is sufficient.

At a distance of six feet, this flash light, I find, is equal in its actinic effects on the plate to the ordinary ribbon magnesium lamp running thirty seconds, with a ribbon one-eighth of an inch in width and burning twelve inches of ribbon.

At twelve feet distance of camera, the flash light is equal to the exposure with magnesium lamp of two minutes and burning four feet of ribbon.

At greater distances than twelve feet my experiments of comparison are indefinite. If a white screen is placed on one side of the object, opposite the side on which the flash is made and a white screen or mirror behind the flash, the illumination is improved and the shadow is less dense. The same result can be obtained by having on the other side of the camera fifteen grains of magnesium with five grains of sulphur and igniting them both at the same time by a spark from a small Ruhmkorff coil. An intervening screen of translucent cloth or paper or ground glass between the the flash and object, will also give fine results but more magnesium is required to be burned as some of the light is obstructed. Exposures by day, when the time is less than one-tenth of a second, should be made from 11 A. M. to 2 P. M. in clear sunshine. The sensitive plate used for instantaneous work should not be less than 25 sensitometer and fewer failures will be made with number 30 or 35. One of the photographs that I will show you is a street view and U. S. Custom House in Charleston, that I took after the earthquake at 12 M., on Sept. 16, 1886, on a 25 sensitometer plate with a single achromatic lens and $\frac{f}{15}$ diaphragm, clear sunshine, estimated time $\frac{1}{20}$ of a second, using a metal drop shutter. Another photograph is a street scene in Washington, corner $4\frac{1}{2}$ Street and Pennsylvania Ave., taken at 1 P. M., July 10, 1887, on a 35 sensitometer plate with a double achromatic lens, $\frac{f}{12}$ diaphragm, clear sunshine, time $\frac{1}{200}$ of a second, using a Prosch shutter. The moving objects in this photograph appear as if at rest in the attitudes seen. Another photograph is one that I took of a snow scene in Washington, corner of 7th and Pennsylvania avenue, at 11 A. M. April 1, 1887, on a 25 sensitometer plate, with single achromatic lens and $\frac{f}{15}$ diaphragm; thin clouds and snowing slightly, estimated time $\frac{1}{10}$ of a second, using a drop shutter with rubber band accelerator.

The shorter the time in which a photograph of moving objects is taken when a good impression can be obtained on a plate the more perfect the photograph. Professor Maybridge has taken photographs of moving objects in the $\frac{1}{5000}$ part of a second.

Distance and velocity of moving objects must be taken into account in successful instantaneous photography. With a lens of five-inch focus and object 1,000 feet distant moving at ten miles per hour, the image on the sensitive plate will change $\frac{73}{100}$ of an inch in one second. A railroad train, moving at

the rate of forty miles an hour, at the distance of four hundred feet, or a man walking two and one-half miles per hour, at twenty-five feet distant, will change the image on the plate the same amount in the same time. The change of over one-half an inch in the image will make a mixed blur of objects, but if the time is reduced to the $\frac{1}{200}$ of a second, the image will then only change the .0036 of an inch, and this change of less than the .0004 of an inch will give a distinct image. The actual motion of a railroad train at forty miles per hour is fifty-eight feet and eight inches per second. A street car at ten miles per hour is fourteen feet and eight inches and a man walking at the rate of two and one-half miles per hour is three feet and eight inches per second.

The method of calculating the change of image on the plate is the same as the lever; the long arm being the distance from the moving object to the lens and the short arm the focus of the lens.

The photograph of a lightning flash can be taken at night by adjusting the camera for distant objects by daylight and then at night pointing the camera with the cap off toward the thunder cloud and await the flash. A single achromatic lens is the best to use. A good photograph of a lightning flash that I will show you was taken by Prof. C. F. Marvin of the U. S. Signal Office, June 19, 1887, at 3 A. M., taken on a Carbutt plate, No. 24 sensitometer, with double achromatic lens, 10-inch focus, $\frac{f}{10}$ diaphragm. The camera was left open four minutes during which time several successive perpendicular and horizontal flashes were photographed on the plate. The exposure was made in Washington, corner of 18th and S streets, N. W. The building photographed on the plate by the lightning flash is Howard University. Good photographs can be taken of the sparks from an induction machine having quart jars in connection with the prime conductors and using a short focus lens.

The photographs exhibited in rapid succession were taken by myself with a 5-inch focus lens and $\frac{f}{6}$ diaphragm on a 35-sensitometer plate.

One photograph sent me from the U. S. training school at Willett's Point, New York harbor, is that of a mule with his head blown off with a dynamite cartridge and photographed before falling to the earth. Taken with a 16-inch focus lens, $\frac{f}{32}$ diaphragm, on an Eastman plate, sensitometer 22, 2 P. M., sunshine and light clouds. Another photograph taken by Prof. C. W. Smiley of the National Museum, Washington, is a mackerel schooner under full sail, taken from the deck of a steamer going in the opposite direction. Lens 16½-inch focus, diaphragm $\frac{f}{16}$, exposure $\frac{1}{100}$ of a second, plate sensitometer 25, sunshine, 12 M. A photograph of base ball playing, showing balls in the air, was taken by Mr. C. C. Jones, assistant photographer U. S. surveys, with a detective camera of 6-inch focus, diaphragm $\frac{f}{24}$ at 4.30 P. M., July 15, time $\frac{1}{60}$ of a second, plate sensitometer 26.

The sensitive plates used in instantaneous photography, particularly the special orthochromatic, must be changed and developed in the least possible amount of actinic light or the negatives will not be clear and distinct. The record I have given of the practical experience in instantaneous photography may be of service to many in their scientific investigations.

DESCRIPTION OF A NEW AND IMPROVED DISSOLVER FOR THE MAGIC LANTERN WITH EXHIBITION OF SLIDES. By Dr. E. P. HOWLAND, Washington, D. C.

[ABSTRACT.]

As the magic lantern has got to be the most important philosophical instrument for scientific and popular illustrations, any improvement in its constructions or arrangement for its manipulation is of scientific and public interest.

A complete dissolver for the lantern is one in which the quantity of the gases for each lantern used is regulated by separate needle valves and independent of the dissolving cock.

There should be no diminution or increase of light in any lantern caused by increasing or diminishing the supply of gases to or from another lantern. It should be so constructed that all the lanterns used could be in full light at the same time or any one or more of them extinguished or relighted without affecting the light of the other lanterns. Most of the dissolvers on the market are constructed for using with two lanterns only, and take the gases from the first lantern to illuminate the second, thereby diminishing the light from the first picture before the second gets fully illuminated, making a dark point between the change of the pictures. The advantages of a good dissolver, besides not having the picture darken down in dissolving, is so that we can have as many pictures on the screen at the same time as we have lanterns and have them all equally illuminated. We then can have different illustrations on the screen side by side, or a picture with figures dissolved in, or a background of different or changing colors with many beautiful and artistic arrangements.

A great variety of beautiful and interesting lantern slides were shown with two, three and four lanterns dissolving.

SPECTRO-PHOTOMETRIC COMPARISON OF SOURCES OF ARTIFICIAL ILLUMINATION. By Dr. E. L. NICHOLS, Cornell University, Ithaca, N. Y., and W. S. FRANKLIN, University of Kansas, Lawrence, Kan.

ON THE RADIATION OF HEAT BETWEEN METALS BY INDUCTION AND BY CONDUCTION, WITH NUMERICAL RESULTS FOR STEEL AND BRASS. By Prof. WM. A. ROGERS, Colby University, Waterville, Me.

ON THE DEFINITIONS OF THE TERMS WEIGHT AND MASS. By Prof. WM. A. ROGERS, Colby University, Waterville, Me.

THE CAUSES OF SUDDEN VARIATIONS OF ATMOSPHERIC PRESSURE. By Dr. M. A. VEEDER, Lyons, New York.

DESCRIPTION OF APPARATUS FOR MAKING A LIGHT-WAVE THE STANDARD OF LENGTH. By Prof. A. A. MICHELSON, U. S. N., and Prof. EDWARD W. MORLEY, Cleveland, Ohio.

TWO STROKES OF LIGHTNING. By Prof. J. W. MOORE, Lafayette College, Easton, Penn.

A GALVANOMETER FOR THE VERTICAL LANTERN. By Prof. J. W. MOORE, Lafayette College, Easton, Penn.

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ADDRESS

BY

PROFESSOR CHARLES E. MUNROE,

VICE PRESIDENT, SECTION C.

SOME PHASES IN THE PROGRESS OF CHEMISTRY.

I am deeply impressed with the honor which you have conferred upon me in selecting me to preside over your section during the present session; but, as I look back upon the eminent men and distinguished chemists who have preceded me in this office, and around upon those who form this section, I feel unequal to the responsibility which the position entails and the obligations which it imposes, and especially so as regards your annual address. For where one is actively and constantly engaged in a variety of pursuits of a highly technical nature, it is difficult to possess one's self of but a very partial knowledge of the advances made in chemistry, since in these busy modern days the science has developed so many special phases, and has shown in many directions so marked a physical and mathematical tendency; while, at the same time, its investigators have continued to add an enormous annual increase to the bodies with which chemistry is to deal. Hence, in reviewing the progress made in chemistry, I must content myself with a glance at some few phases and leave many as important ones untouched.

Since the isolation and recognition of oxygen by Priestley, the search for new elements, like that for new heavenly bodies, has

formed for many a most entertaining pursuit, the facilities for which have been much increased by the use of the spectroscope and the delicate spectroscopic methods as developed by Bunsen and Kirchoff, and perfected by Nobert, Rutherford and Rowland in construction, and by Crookes, Boisbaudron, Liveing and Dewar and others in methods, and the result has been to extend continually the list of bodies which are grouped under this head. The announcement of new discoveries during the last ten years has been especially large. According to a recent statement of Dr. Bolton,[1] who has kept a careful record of these announcements, over seventy bodies have been added to the list during this time, though it will be observed that many of these so-called elements are obtained by the resolution of others which are included in the above-mentioned number. The largest number added by any observer has resulted from the joint labors of Krüss and Nilson[2] on the absorption spectra of the rare earths and reaches to over twenty. Should these discoveries be verified and the elementary character of the substances, as we now use the term elementary, be established, the possible number of compounds which would result is something enormous; but, judging from experience, few of them are likely to survive a very searching criticism. Still two in this list, scandium and germanium, have already passed the tests, and the latter, which was discovered by Winkler in 1886, has been accepted as the missing element in Mendeléeff's scheme, whose existence and properties he predicted under the name of ekasilicon.

Among the fundamental constants of chemistry there are none which occupy a more important place than the weights of the atoms of the elementary substances; and it is only natural that, from the time when the present chemical elements were recognized as such, and especially since the adoption of the atomic hypothesis as enunciated by Dalton, strenuous efforts should have been made to determine this constant for each element with all the precision of which chemical art permits. Apart from the evident necessity and advantage of knowing this constant for purposes of analysis and in chemical processes in general, an added zest was given to the pursuit by the hypothesis of evolution as developed by Prout, and extended and modified by Dobereiner, Dumas, Cooke, Pettenkofer, Odling and Gladstone in their demonstrations of the numerical

[1] J. Anal. Chem., **2**, *282-283;* 1888. [2] Ber. d. chem. Ges., **20**, *2134;* 1887.

relations existing between the atomic weights of elements belonging to the same natural group; while, more recently, a deeper interest has been imparted through the discussions by Newlands, Mendeléeff, Lothar Meyer, Carnelley, Mills, Reynolds and others of the data already collected which leads to the conclusion that the properties of the elements are functions of their atomic weights and that the various weights are related according to some law of nature. Thanks to the labors of Becker, Clarke, Lothar Meyer and Seubert, the literature of the subject of the determinations of the atomic weights has been carefully reviewed, the processes collated and the data discussed so that the whole matter is now readily accessible to any one who desires to engage in further research in this field.

Since the unit weight of hydrogen is taken as the standard for comparison, while the determination of the atomic weights of a large number of the elements has been made only through the intervention of oxygen, the ratio of the atomic weights of these two elements is the most important one to be determined, for any error which may occur here will be magnified when repeated through a moderate series of other ratios. Three methods were employed by the earlier investigators for fixing this constant: (1) through the synthesis of water, which was effected by passing hydrogen gas over hot copper oxide; (2) through the exact determination of the relative densities of the two gases; (3) by weighing the quantity of water formed through the direct union of a known volume of hydrogen with oxygen. The first method was employed by Dulong and Berzelius, by Dumas, and by Erdmann and Marchand; the second by Dumas and Boussingault, and by Regnault; and the third by J. Thomsen. Of these researches, that of Dumas by the first method is by far the most important, and it constitutes one of the most memorable investigations in the history of chemistry. In this he burnt an undetermined amount of hydrogen by means of copper oxide, the amount of oxygen consumed being determined by the loss of weight of the tube containing the copper oxide, and the water formed being collected and weighed directly. The greatest care was taken to insure the purity of all the materials used, every known experimental means were employed to secure accuracy, and all the corrections which could be conceived of were applied to the results, while the experiments were carried out on a very large scale, the amount of water produced being in some

cases as high as seventy grams. As a result, the value of the atomic weight of oxygen was, as the mean of nineteen determinations, found to be 15.9607 with a probable error of $\pm .007$.

Notwithstanding the great intelligence and skill displayed by Dumas in the devising and execution of this research, the process is open to serious criticism, inasmuch as that the weight of the hydrogen in the water obtained is estimated by difference and hence any errors in the process are accumulated in the value obtained for the hydrogen and as this is the lighter body such errors may become very appreciable. Dumas himself recognized this, for he says: "Of all analyses presented to the chemist, that of water is the one which offers the greatest uncertainty. Indeed, one part of hydrogen unites with eight parts of oxygen to form water, and nothing would be more exact than the analysis of water if we could weigh the hydrogen as well as the water which results from its combustion. But the experiment is not possible under this form. We are obliged to weigh the water formed, and the oxygen which was consumed in producing it, and to determine by difference the weight of the hydrogen which has entered into combination. Thus an error of $\frac{1}{900}$ in the weight of the water, or of $\frac{1}{800}$ in the weight of the oxygen, is equivalent to an error of $\frac{1}{80}$ or $\frac{1}{90}$ in the weight of the hydrogen. Let these two errors be in the same direction and the total error will amount to $\frac{1}{40}$."

Dr. J. P. Cooke[1] and T. W. Richards have, however, sought to solve this problem and have contrived a very ingenious apparatus by which the weight of hydrogen burnt and of water obtained is known, while the weight of the oxygen contained is estimated by difference. For this purpose, pure, dry hydrogen was first collected in a large, glass balloon, which had previously been exhausted and weighed, and then carefully weighed again, the container being counterpoised by a second balloon of similar material and of exactly the same external volume. The balloon used had a capacity of 4961.5 cubic centimetres, weighed 570.5 grams and held about .42 grams of hydrogen. The method of compensation adopted in the weighing of the large globe was found to be so accurate that under good conditions the weight of the globe, when filled, did not vary more than one-tenth of a milligram through large changes of temperature and pressure. The filled and weighed

[1] Amer. Chem. Journ., 10, *81-110*, 1888.

balloon was then inserted between a set of combustion furnaces so arranged that pure, dry atmospheric nitrogen should be drawn into the balloon so as to sweep the hydrogen before it. The hydrogen was thus passed over ignited copper oxide and the water produced collected in suitable condensers. Finally, pure, dry air was drawn through the whole apparatus, so that at the end of the operation the apparatus and contents were in the same condition as at the start except that the hydrogen was all in the form of water. The operation was conducted with all the precision possible, and the mean of the fifteen determinations gave 15.953 ± 0.0017 as the atomic weight of oxygen.

Dr. Scott[1] has repeated the experiments of Gay-Lussac and Humboldt for ascertaining the composition of water by volume, using an improved eudiometer in the place of the Volta eudiometer used by them, and besides seeking to attain greater accuracy by preparing purer gases, using larger volumes, measuring both gases in the same vessel and by analyzing the residue after each explosion and determining the impurities present in each experiment. The apparatus used was entirely of glass with the exception of the rubber connections to the receivers employed for holding the mercury used in exhausting the apparatus. The oxygen employed was produced from potassium chlorate or mercuric oxide and the hydrogen by electrolysis. The largest amount of impurity found present in any one of the twenty-one experiments was $\frac{1}{202}$ parts while it fell in another case to $\frac{1}{2887}$ parts. As a result of these experiments Scott finds the most probable ratio of the gases by volume in water to be $H : O = 1.994 : 1$. Hence taking the density of oxygen referred to hydrogen as 15.9627, he arrives at an atomic weight for oxygen of 16.01. A more recent determination of this ratio by Scott under improved conditions gave $H : O = 1.9965 : 1$.

In 1882 Lord Rayleigh, animated by the same motive as Dr. Cooke, namely, the desire to examine whether the relative atomic weights of hydrogen and oxygen really deviated from the simple ratio of 1 :16 as demanded by Prout's hypothesis, planned an investigation for determining the relative densities of these two gases, the results of which he held when combined with the determinations of the relative atomic volumes, measured by eudiometric methods such as Mr. Scott employed, ought to lead to a true

[1] Chem. News, 56, *173–175*, 1887.

solution; for, if both investigations were conducted with gases under the normal atmospheric conditions as to temperature and pressure, any small departures from the laws of Boyle and Charles would be practically without influence upon the final number representing the atomic weights.

A preliminary notice of this investigation has now appeared[1] from which we learn that Lord Rayleigh used a large balloon for holding and weighing his gases just as Regnault and Cooke previously had, and he discovered that in both cases an error had been introduced into the experiments owing to the failure to take into account the contraction which took place in the balloon when it was exhausted. Naturally, the balloon then displaced less air than when filled, and an error results if the simple difference of the weights of empty and filled globes be taken as the weight of the contained gas. Special attention was paid to the method of producing the hydrogen and of purifying the gas, while the apparatus connecting the generator with the receiver was constructed with fused glass joints and sealed taps, there being but one rubber connection and the leakage at this being determined at the beginning of the operation by means of a Töppler pump. By this method and applying all the necessary corrections apparent, Rayleigh finds the ratio of the densities to be H : O = 1 : 15.884 and combining this with Scott's latest determination for the relative atomic volumes, we have for the ratio of the atomic weights H : O = 1 : 15.912.

In a subsequent paper[2] Cooke and Richards have confirmed by experiment the observation made by Rayleigh as to the contraction of the balloon used by them; and, on applying this correction to their previous result, they arrive at a value of 15.869 ± 0.0017 for the atomic weight of oxygen.

Dr. Keiser[3] has also attacked this problem and has started with weighed quantities of hydrogen; but, instead of using large balloons in which to hold the gas, he has taken advantage of the singular property of palladium for occluding hydrogen to secure a container in which it may be conveniently and accurately weighed. The property of palladium to occlude hydrogen was discovered by Graham in 1868 and he showed that at ordinary temperatures it occluded

[1] Chem. News, 57, *73-75;* 1888. [2] Am. Chem. J., 10, *191-196;* 1888.
[3] Am. Chem. J., 10, *249-261;* 1888.

many hundred times its own volume of hydrogen gas. The palladium-hydrogen which is thus formed is stable at ordinary temperatures and the metal retains the hydrogen even in a vacuum. If, however, the temperature be raised above 100°, then the gas will be expelled in a slow and regular current. Under atmospheric pressure nearly all of the hydrogen can be driven out at temperatures below 200°. By enclosing a quantity of metallic palladium in a glass vessel from which the air has been removed and saturating it with pure hydrogen, a large volume of the gas may be condensed into a small space. If now the temperature be raised gradually, the gas will be driven out slowly and regularly. If the vessel be weighed before and after the heating, the loss in weight will equal the weight of hydrogen that has been expelled. It is possible, therefore, with an apparatus of small dimensions to weigh comparatively large volumes of hydrogen. Preliminary experiments showed that 100 grams of palladium foil will readily absorb from .6 to .7 grams of hydrogen, and that with an apparatus having a volume of about 150 cm^3. and weighing 130 grams, it is possible to weigh, with great accuracy, an amount of hydrogen which in the gaseous state would occupy a volume of from 7 to 8 litres. Besides the precision gained in weighing, Keiser claims, that, by the use of palladium, the hydrogen may be obtained in a greater state of purity, since palladium possesses a selective power for hydrogen when mixed with other gases or even when in combination, as in some of the hydrocarbons, and this claim seems supported by the experiments of Graham, Wöhler, Wilm and Hempel together with those of his own.

The apparatus employed for the combustion involved a considerable number of rubber connections, but these were varnished and securely wired. By this method Keiser obtained as the mean of ten determinations a value of 15.9492 for the atomic weight of hydrogen.

Morley, whose admirable researches upon the composition of the atmosphere, and upon the drying of gases by sulphuric acid and phosphorus pentoxide, are so well and favorably known, has proposed[1] several promising plans for the determination of this important ratio, which he has been developing for some years and to which the researches spoken of may be regarded as preliminary, it

[1] Am. Chem. J., 10, *21-26*; 1888.

is sincerely to be hoped that his health may soon permit him to carry them to a successful issue.

While presenting to the British Association a preliminary notice[1] of his experiments for determining the atomic weight of gold, Prof. Mallet suggested that the most important direction in which an advance in our knowledge of the atomic weights was to be made was by endeavoring to eliminate "constant errors," as distinguished from mere personal or casual errors of experiment, from our results. We have been taught by the example of Stas to reduce the latter to very small values by minute and elaborate precautions, but the former are always to be suspected, and all conceivable means should be used to avoid them. The following were pointed out as among the most important of such means:—

(1) In every case resort to "fractional" methods for the purification of the materials to be used, assuming the materials to be pure only when earlier and later fractions give sensibly identical results.

(2) Use great care in the study of the reactions which are depended upon for the final determinations of the atomic weights, looking especially to any possibility of the occurrence of secondary or subsidiary reactions.

(3) Adopt methods by which (*a*) the atomic weight to be determined may be connected *directly* with that of hydrogen, or (*b*), if connected indirectly by the intervention in each single determination of as *few* other elements, but in determinations by different methods of as *many* other elements as possible of supposed well-known atomic weight.

Dr. Mallet has devised a method for determining the atomic weight of gold by which the final weight may be directly connected with hydrogen. The volume of hydrogen which is evolved by a given weight of zinc of a high degree of purity when dissolved in dilute sulphuric acid is first determined and a solution of auric chloride or bromide is then treated with an excess of the same zinc. When the gold in solution ceases to precipitate, the excess of zinc is dissolved in dilute sulphuric acid and the hydrogen evolved measured. The precipitated gold is collected and weighed.

The difference between the volume of hydrogen which the zinc gives when partly used to replace a known quantity of gold and when used in replacing hydrogen only, taken in connection with

[1] Chem. News, 56, *132*; 1887.

Regnault's determination of the relation of weight to volume for hydrogen, affords the data needed.

An advantage in this method consists in the fact that neither the weight of the gold salt in solution, nor the atomic weight of the halogen combined with the gold, nor the atomic weight of the zinc need be known, nor need the zinc used be of assured purity provided only that it be uniform in character so that a given weight of it can be depended on to yield always the same quantity of hydrogen, and there be no impurities present capable of interfering with the collection of the precipitated metallic gold in a state of purity.

Of course the error in Regnault's determination, as pointed out by Lord Rayleigh, must be corrected for here and in fact must be applied in the revision of many of the existing determinations of the weights of the various elements. The same holds true for the multitude of determinations of physical constants in which Regnault's data have been employed in the estimation of the final values.

I have dwelt at length upon these important researches, not alone because of their individual intrinsic value and interest, but because they, as well as many other recent determinations of atomic weights which might be cited, show that even yet, with all the advantages of purity of material, perfection of apparatus and precision of methods, united to great skill and extensive attainments on the part of the experimenters, the attractive hypothesis of Prout remains experimentally unproved. Many hold that the failure in the proof has been due to constant errors in the experimental processes, but Meyer and Seubert,[1] from an elaborate discussion of the determinations of the atomic weight of silver and of those of the other more important elements calculated by its means, declare that they all contradict Prout's hypothesis in its characteristic original conception and that it must therefore be looked upon as having been disproved by experiment.

Crookes[2] suggests a hypothesis which may account for certain of the discrepancies in the atomic weight determinations without resorting to the supposition of constant errors. He supposes that elements, instead of being composed of parts of matter which are identical throughout, are really composed of groups of particles which are only approximately alike and whose weights only approximate to that average which we call the atomic weight. Hence it is

[1] Ber. d. chem. Ges., 18, *1098*; 1885. [2] Chem. News, 54, *122*; 1886.

possible that in different portions of such congeries different average values within small limits may obtain. Granting this, it is remarkable that such close coincidences should result as have resulted from the observations made on material obtained from widely separated sources.

The determination of molecular weights is of nearly equal importance with that of the weights of the atoms, for the validity of our modern theories of chemistry greatly depends upon the exactness with which the molecular magnitudes are defined. It is of course comparatively easy to ascertain by analysis the proportions in which the elementary substances exist in the compound and from this data to calculate an empirical formula which satisfies, but which may be but some multiple of the true molecular formula. Thanks to Avogadro's law we are able, when the substance can be obtained in the gaseous state, to determine which among the derived expressions is the true one; for we have only to determine the vapor density by the methods of Dumas, or Hofmann, or Victor Meyer, by the diffusion method of Graham, or by others which have been suggested, and the number found is a close approximation to one-half the true molecular weight. When, however, the body could not be completely volatilized unchanged at temperatures conveniently realized we have, until recently, been dependent upon isomorphism and the laws of molecular volumes and of specific heats and upon analogical comparisons to furnish us with estimates of the molecular weights.

When the bodies are volatile, but at temperatures so high that our methods for determining the vapor densities are unavailable, we may avail ourselves of this function as a measure of the molecular weights; for numerous observations have shown that the relative volatilities of substances depend not only on the composition of the bodies but also on their molecular weights, and in general other things being equal the volatility increases inversely as the molecular weight. Unfortunately from the study of volatile liquids it has been found that among metameric bodies the rule is modified and the effect of the molecular grouping of the atoms becomes noticeable, while in some isologous series the rule is reversed, so that the method has but a limited application while the results at the best give but rough approximations to the true molecular weights.

It is held that "a closer investigation of the internal movements of liquids especially friction, diffusion and conductivity of heat

will yield the safest means of determining the weights of the molecules which actually exist in the liquid state. But the theory of these phenomena would require a special development based on a definite idea of the properties of the molecule before it would be possible to draw conclusions as to the values for the molecules from the observations which have been and have yet to be made."

In 1788 Blagden showed that when inorganic salts are dissolved in water the freezing points of the solutions are reduced by an amount proportional to the weight of the substance dissolved in a constant weight of water. De Coppet, in 1871–72, pointed out that when this diminution is calculated for a determinate quantity of the substance dissolved in 100 grams of water the result, which he terms the coëfficient of depression, is constant for the same substance, and that the coëfficients for different substances bear a simple relation to their molecular weights. Raoult has shown what these relations are, has extended the investigation to organic substances and to other solvents than water, and has deduced a formula, by which, knowing the weights of the substance employed and of the solvent and the depression of the freezing point, the molecular weight may be calculated. He has examined a large number of substances whose molecular weights had previously been determined by their vapor densities and the results obtained illustrate in a remarkable manner the accuracy and general applicability of this new method. Paterno and Hasini,[1] Victor Meyer,[2] Auwers,[3] Brown and Morris[4] and others have applied the method and find it in general to hold, especially when acetic acid is used as the solvent.

The process is very simple for it requires only a small beaker or test tube which is closed by a cork perforated with three openings through one of which a thermometer is passed, through the second a stirrer is passed and through the third a bit of the solid mixture is dropped in order to produce congelation. A weighed quantity of the solution of known proportions is placed in the vessel, the whole immersed in the freezing mixture, the bit of solid dropped in and the temperature of freezing noted.

Ramsay[1] has applied this method to the determination of the much discussed question of the molecular weight of nitric peroxide in

[1]Ber. d. chem. Ges., 19, *2527*; 1886. [2]Ber. d. chem. Ges., 20, *556*; 1887.
[3]Ber. d. chem. Ges., 20, *1860*; 1887. [4]J. Chem. Soc., 52, *610–621*; 1888.
[5] J. Chem. Soc., 52, *621–623*; 1888.

the liquid state and he obtains a value which leads to the formula of N_2O_4.

There has long existed a conviction in the minds of many that the molecular constitution of bodies in the solid state was much more complex than in the gaseous, owing to polymerization, and the opinion finds support in the diminishing density and increasing molecular simplicity of such bodies as acetic acid and sulphur when subjected to high temperatures, or as has been more recently shown by Mensching and Victor Meyer[1] in the case of phosphorus and arsenic, which when at a white heat give densities equivalent to the formulas P_2 and As_2 instead of those more complex formulas which have been generally accepted up to this time.

By analogy this condensation or aggregation of molecules should proceed as we pass from the gaseous, through the liquid, the to solid state. Is it not singular that the molecular weights derived by Raoult's method for bodies in a state of solution should be identical or approximately so with those deduced from their densities in the state of a gas? This method fails to afford any indication whatever of this molecular complexity in solids and liquids. Must it not then be assumed that the solvent has effected the complete dissociation of the complex molecules present in it? If so, this probably extends to all cases of true solution without chemical action, if such there be, and this is assumed in this method, for although the solvent used has been varied, all have given similar results.

Until recently we have known little precisely about the nature of solution. It has been held by some that the phenomena of solution differ essentially from those chemical combination, inasmuch as in the former we have to do with gradual increase up to a given limit, termed the point of saturation, whereas in the latter we observe the occurrence of constant definite proportions in which, and in no others, combination occurs. "Solution obeys a law of continuity, chemical combination, one of sudden change or discontinuity."

But solution is often attended with a thermal change and from many considerations it would appear that the process is attended with the development of both chemical and physical phenomena; for by the addition of water to a concentrated solution of a chemical compound, a separation of the compound into two or more con-

[1]Ber. d. chem. Ges., 20—; 1883.

stituents is sometimes effected, the amount of chemical change being frequently dependent on the relative masses of the water and the dissolved compound and the temperature. If the masses are kept constant, the amount of chemical change, in some cases, increases as the temperature rises, and the original compound is re-formed on cooling.

This decomposition may proceed even at ordinary or below the ordinary temperatures, as for instance in a saturated solution of ammonium acetate, which loses ammonia when a current of hydrogen is passed through it even at 0°, showing that the solution probably contains free ammonia and, if so, free acetic acid also; or in an aqueous solution of ferric chloride, which undergoes partial separation into hydrochloric acid and a soluble form of ferric hydroxide, the amount of separation increasing as the dilution increases.

"Analogies have been traced between such changes as these and processes of dissociation. But even the simplest case of so-called dissociation in solution presents much more complex phenomena than a case of true dissociation. The action of water has been compared to that of pressure; dilution has been regarded as analogous to decreased pressure. In most of these so-called dissociations, water is one of the constituents of the dissociating system; hence, judging from the supposed analogy, we should expect the process to become slower and finally stop as the quantity of water increases. But the reverse of this actually occurs. The water probably exerts two actions: one which may be called physical whereby an increase in the quantity of water gives greater freedom of motion to the particles of dissolved substance, and also lessens the chances of combination between the separated constituents of the substance; and another, which may be called chemical, whereby an increase in the quantity of water determines the formation or decomposition of definite compounds which would not otherwise be produced. While an increase in the quantity of water may in one respect tend to increase the amount of chemical change, it may, in the other respect, exert an influence in the opposite direction. When the physical is more marked than the chemical action of water, we shall have phenomena resembling those presented in gaseous dissociation."[1]

Some of the advocates of the chemical view have urged that solution was attended with the formation of hydrates varying with

[1] Elements of Thermal Chemistry, Muir and Wilson.

the dilution. This hydrate theory has been studied by Thomsen from a thermo-chemical standpoint, and considering the whole of his investigation on the heats of solution and dilution of acids, alkalies and salts, he holds that the results he has obtained are altogether opposed to the supposition that aqueous solutions of these compounds contain various hydrates differing in composition with the amount of water present. His results rather point to the hypothesis that when an acid, an alkali, or a salt, dissolves in water, either a hydrate is formed and dissolves as such, or the compound is dissolved without combination with the water, and that an increase in the quantity of water does not affect the chemical composition of the solution. At the same time dilution is always attended with a thermal change, the cause for which he finds in the changes of the states of motion of the particles of the solutions. When two liquids mix without any tendency to separation, it is suggested that such changes in the motion of the two kinds of particles occur as result in the formation of a homogeneous liquid, the particles of which are in equilibrium because the mean motions of the different particles are the same. But this mutual accommodation of the particles is accompanied by a transference of energy from one kind to another, and it is to such transferences of energy that the cause of the thermal changes is to be referred.

In a series of valuable papers, W. W. J. Nicol has also sought to show that the hydrate theory is untenable, and his conclusions have met with approval.

Mendeléeff[1] has recently put forward the following hypothesis : solutions may be regarded as strictly definite, atomic, chemical combinations at temperatures higher than their dissociation temperatures. Definite chemical substances may be either formed or decomposed at temperatures which are higher than those at which dissociation commences ; the same phenomenon occurs in solutions ; at ordinary temperatures they can be either formed or decomposed. In addition, the equilibrium between the quantity of the definite compound and of its products of dissociation is defined by the laws of chemical equilibrium, which laws require a relation between equal volumes and their dependence on the mass of the active component parts. Therefore, if the above hypothesis of solution be correct, comparisons must be made of equal volumes ; the specific gravities are the weights of equal volumes, and moreover we

[1] J. Chem. Soc., 51, 778-782; 1887.

must expect the specific gravities of solutions to depend on the extent to which the active substances are produced; therefore, the expression for specific gravity s, as a function of the percentage composition p, must be a parabola of the second order, while between two definite compounds which exist in solutions we must expect that the differential coëfficient $\frac{ds}{dp}$ will be a rectilinear function of p. This consequence can be verified by experiment while at the same time it affords the means for ascertaining what are the definite combinations existing in the solution. Applying this method to the solution of water and alcohol he finds three definite combinations to exist, two of which he isolated and he demonstrates graphically the rectilinear character of the differential coëfficient.

This hypothesis respecting the rectilinear character of the differential coëfficient has been proved by Mendeléeff to be correct, not only for solutions of a hundred different salts but also for solutions of H_2SO_4, NH_3, HCl and similar substances, and not a single exception has been met with.

Crompton[1] has extended this theory of solution to the discussion of the electrical conductivity of aqueous solutions; taking for this purpose sulphuric acid and a number of other typical substances. In a recent paper Mendeléeff has concluded, from a discussion of the data for sulphuric acid, that this acid forms four hydrates and, of the four deduced, two were already recognized by chemists. Now taking the curves which show the relation between electrical conductivity and percentage composition, plotted from data supplied by Kohlrausch, Crompton sought, by treating the electrical conductivity as a function of the percentage composition and differentiating in Mendeléeff's manner, to determine if this curve could not be broken up so as to give evidence of the definite hydrates above mentioned. The first differential coëfficient failed to do this; but assuming that the function might be of the third order, and taking the second differential coëfficient, a curve resulted which consisted of a series of rectilinear curves showing breaks which occurred at the points corresponding with the composition of the hydrates discovered by Mendeléeff, with the addition of one other.

This most important concurrence, with Mendeléeff's result, besides helping to confirm the latter proves that the electrical conductivity of sulphuric acid, if not wholly due to, is largely influenced by, the formation of definite hydrates in solution; and that the recti-

[1] Chem. J. Soc., 53, 116-125: 1888.

linear character of the second differential coëfficient curve gives us the means of ascertaining what hydrates there are which exercise this influence.

At the Montreal meeting of the B. A. A. the President, Lord Rayleigh, said in his address, "From the further study of electrolysis we may expect to gain improved views as to the nature of the chemical reactions and of the forces concerned in bringing them about . . . . . . I cannot help thinking that the next great advance, of which we have already some foreshadowing, will come on this side. And if I might without presumption venture a word of recommendation, it would be in favor of a more minute study of the simple chemical phenomena."

In presenting his theory on electrolytic conduction, in which he holds that in the case of composite electrolytes, such as mixtures of sulphuric acid, or hydrogen chloride and the like with water, electrolysis is the outcome of the combined action of the E. M. F. and of some effect which the one set of molecules exerts upon the other set, while both are under the influence of the E. M. F., Dr. Armstrong[1] contends that, thanks to Mendeléeff, the great advance of which Lord Rayleigh spoke is no longer far distant, and that it is patent that electrolysis is primarily an affair of molecules; that electrolysis takes place in consequence of an influence which one set of molecules, *A*, exercises upon another set of molecules, *B*. The results which Mr. Crompton has obtained are to his mind conclusive on this point, the information afforded by the study of sulphuric acid solutions being alone sufficient; and they are of special importance as indicating the superiority of electrical values over all others in any discussion of the constitution of complex systems of dissociable compounds. This was to be expected, as, on the hypothesis under discussion, the variations in the electrical values would represent variations in the extent to which the one set of molecules affect the other set, the electrical values serving in fact to quantify an influence, for changes in constitution or structure might well occur which would involve but a slight degradation of energy and consequently a slight change in density and many other physical properties, and which yet might lead to a relatively very considerable change in the extent to which the compound could exert an influence on the course of electrolytic or chemical change. In the case of sulphuric acid the evidence is

[1] J. Chem. Soc., 52, *125-138;* 1888.

all but complete, and it is no exaggeration to say that every peculiarity of the acid is faithfully pictured in the second differential coefficient curve.

Mendeléeff's hypothesis is supported also by the results obtained by Pickering in his study of the heat evolved by the dilution of solutions of calcium chloride. Taking temperature and percentage composition as his factors he has found a second differentiation necessary to reduce the original curves to straight lines. Owing to the multiplicity of the breaks and the smallness in percentage differences corresponding to each additional molecule of H_2O, it was extremely difficult, in some instances, to locate the exact position of breaks; but by taking the densities of the solutions, and treating the curve as Mendeléeff had done, the points at which the breaks occurred in the curve corresponded precisely with those in the heat curve, and they corresponded as accurately as could be expected with definite molecular proportions of water.

Judging from the inspection of the periodical literature, Lord Rayleigh in recommending the more minute study of the simpler chemical phenomena was but voicing an opinion which had formed itself in the minds of many investigators, and the results of these efforts show that in the case of many of the most familiar chemical processes, the reactions are quite complex and are dependent upon a variety of conditions. Even the well-known case of the decomposition of potassium chlorate by heat is shown by Teed and by Frankland and Dingwall to be extremely complex, thus confirming the assertions of Maumené. As has long since been accepted for reactions such as take place in mixtures like gunpowder, the equations usually given for the simpler chemical reactions can only represent the initial and final states for a single set of conditions.

Berthollet urged the study of these conditions more than eighty years ago when advancing his views on the nature of affinity, which he held to be a phase of the same fundamental property of matter as that to which universal gravitation owes its origin, the action appearing more complicated than gravity because of the close proximity of the reacting substances; for, under the circumstances the phenomena are influenced not only by the mass and distance of the smallest particles or molecules, but also by their form, their distances from one another and the peculiar conditions under which they exist, and he sought to explain these phenomena through the application to them of the general laws of statics and dynamics.

In his masterly work on the Modern Theories of Chemistry, Dr.

Lothar Meyer shows from the discussion of an immense amount of experimental data, that unless all indications prove deceptive, the immediate future must bring us considerably nearer to a kinetic theory of affinity, if it succeeds, as we may hope, in obtaining a definite notion of the true nature of electricity as a peculiar form of motion.

"The chemical statics, or, as we may now call it, the chemical mechanics which Berthollet created at the beginning of the present century, will receive a totally different shape from the introduction of the hypotheses and theories which have been and which in the future will be generally accepted.

Although all the views and hypotheses from which Berthollet started have undergone transformation, the aim he had in view remains unaltered. It is still the application of the general laws of static and mechanic laws to chemical phenomena.

The best proof that Berthollet was right in his endeavors lies in the fact that the goal he had in view remains unaltered. The completion of his work, which was interrupted for many years, has recommenced, and has been energetically pushed forward in the last few years by means of the excellent methods we now possess. The work is troublesome, but comparatively easy. The old frame will be refilled with the new material which the prosperous development of the science has since yielded and daily continues to yield. The completion of the work will demand much time and energy, but rich will be the reward. It will erect a fitting monument to the glorious genius of the founder."

The progress in analytical and descriptive chemistry and in the production of new compound bodies continues at such a pace and is so fertile in results that the mere enumeration of them would occupy more time than is at my disposal. But we notice as most important the isolation of fluorine by Moissan,[1] after three years of labor; the isolation of the radical substance, hydrazine, by Curtius;[2] and the discovery by Gibbs,[3] of a remarkable series of bodies (with molecular weights reaching to over 20,000, and possessing a molecular complexity which equal if they do not exceed any known to organic chemistry) which have been produced during his researches on the complex inorganic acids. Organic chemistry is now cultivated to an extent, especially in this country, which must be very gratifying to my distinguished predecessor, who, but eight years

[1] Ann. chim. phys. 12 [6] *473–538;* 1887. [2] Ber. d. chem. Ges., 20, *1634*, 1887.
[3] Am. Chem. J., 1879–1887.

ago, made from this chair a sound and urgent plea for its wider recognition. A marked feature of this recent advance has been a more complete effacing of the boundaries which were formerly thought to distinguish inorganic from organic chemistry.

I must pause here in my hasty sketch to note a very curious relation which has been observed by Dr. Blake[1] between the quantivalence of the inorganic elements and their biological action when injected into the circulatory systems of living animals. He has employed salts in which the electro-positive elements varied in valence from one to four, and he found that the biological action extended with the valence, the action of univalent elements being confined to the pulmonary arteries, while with the quadrivalent elements the respiratory, the vasomotor and inhibitory centres, the brain, spinal marrow, cardiac ganglia and pulmonary arteries were all affected. Dr. Blake has already demonstrated the important part played by isomorphism and atomic weight in acting upon living tissue and now regards valency as another important factor. These relations may furnish important assistance in researches in molecular chemistry. It must not be forgotten that when Newlands published the "periodic law" he made the interesting observation that all the elements which are found in organized bodies have atomic weights below forty. It may be now added that all the electro-positive elements found in organized bodies are univalent or bivalent.

Among the many hypotheses concerning matter which have been devised there is probably none, and certainly none in recent times, which has been the subject of more searching criticism than the evolutionary hypothesis. Although the doctrine is, in the main, a very old one and has been presented in various forms for more than three centuries yet it has only met with wide acceptance and almost equally wide antagonism since Darwin and Wallace first published their theories of natural selection, as based on the evolutionary hypothesis, some thirty years ago.

Although the argument of these masters was most powerful and supported by a wonderful array of experimental and observational data yet its success was no doubt in a great measure due to the fact that it was an additional proof of the operation of the law of continuity in nature; the action of which, as set forth in the neb-

[1] Compt. rend., 106, *1260;* 1888.

ular hypothesis, so well explained the origin and development of the heavenly bodies. This noble hypothesis of Kant's which was placed on a secure foundation by La Place and presented in a clear and luminous form, with a masterly grouping of the evidence by Herschel stands to-day as one of the noblest speculations of science and the grandest example of the application of the doctrine of evolution, yet Newcomb is obliged to say of it that "at the present time we can only say that the nebular hypothesis is indicated by the general tendencies of the laws of nature, that it has not been proved to be inconsistent with any fact, and that it is almost a necessary consequence of the only theory by which we can account for the origin and conservation of the sun's heat, but that it rests upon the assumption that this conservation is to be explained by the laws of nature as we now see them in operation. Should any one be skeptical as to the sufficiency of these laws to account for the present state of things, science can furnish no evidence strong enough to overthrow his doubts until the sun shall be found growing smaller by actual measurement, or the nebulæ be actually seen to condense into stars and systems."

In fact it is probable that an enormous period of time must elapse before data can be obtained by direct observations of this kind on our solar system which will prove or disprove this hypothesis. It is true that other methods of research have been employed in the tests of this hypothesis with varying results. Thus it has been found that as more powerful telescopes have been constructed fewer nebulæ have resisted resolution and this evidence has tended to undermine the hypothesis; on the other hand, the evidence furnished by the spectroscope has in general shown an increasing complexity of constitution as we pass from the nebulæ to the suns and this has tended to support the hypothesis. But neither of these yields the final and convincing proof which is demanded.

So too as regards living things although reproduction may go on in certain of the lower orders of organized beings so rapidly, that variations so marked may be produced in successive generations by changes of environment and qualifying conditions as to point strongly to the possibility of the passage from one species to another and hence the common origin of all, yet the absolute proof is wanting, and we are dealing here with such complicated structures and obscure forces that it is a question whether the absolute proof can be obtained by the present methods of research.

In geology and mineralogy too, though many of the changes of the contour of our earth have been observed in actual operation and many of the well-known minerals have been formed artificially, and the transition of several species has actually been traced while taking place in nature, yet here the number of factors involved is so great, and the time factor especially is so large that many breaks must and do exist in the continuous series.

It is apparently to physics and chemistry that we should first look for the experimental facts which prove or disprove the doctrine of evolution as founded upon the law of continuity, for here we deal with matter in its simpler forms and forces in their simpler manifestations, while it is possible to so reduce the factors in our experimental equations that they may be varied with comparative ease and in a comparatively short time, so that the entire operation can be conducted under our own observation. Besides, since all bodies are composed of the matter with which chemistry deals, and affected largely by the forces with which physics deal, it seems the logical method of procedure, when possible, to begin with the study of the fundamental and simpler forms and to build the superstructure of our theory upon the data so collected.

As a fact we already owe to both these sciences the existence of positive evidence of the truth of the doctrine of evolution; for the synthesis or creation of compound bodies and their analysis or disintegration are operations which are being daily conducted in our laboratories and workshops at the will of man, and these changes are not wrought upon inorganic matter only, but bodies such as urea, alcohol, alizarine, vaniline, indigo, and a multitude of others, which are the products of so-called vital processes, have been formed in this way from their elements by purely chemical processes; while by the methods of substitution and metathesis whole series of homologous hydrocarbons, identical with those existing in nature, have been evolved from elementary matter, and further we find in allotropism, isomerism (in its various degrees), and isoallomerism, in the behavior of the compound radicals, and in the changes wrought by changes in the factors of time, mass and temperature, positive evidence of the effect of environment and qualifying conditions in determining the production of species. In truth the evidence supplied is so strong as to have forced the conviction in the minds of many of the ablest chemists that all matter is one and varies only as it is acted upon by force, while on the

other hand the transformations of energy which are continually to be seen occurring in nature and in art as continually prove the truth of that glorious conception, the doctrine of the conservation of energy, and equally force the conviction that all energy is one and varies only in its manifestations.

The belief in the unity of matter is as old as philosophy, and, as has been said, this belief has in recent time been strengthened to conviction by the development of such facts as I have alluded to above, and this conviction has been supported by the more recently discovered evidence that the properties of the elements are functions of their atomic weights and that the elements when arranged according to their atomic weights fall into natural and periodic groups; for it is a fundamental deduction from the law of periodicity that the various elementary atoms must be aggregations or condensations of one and the same primordial substance. Strong however as the conviction resting upon this evidence may be, there is yet lacking the crucial proof; for we have as yet failed to observe the passage of matter from the form of one elementary substance to that of another, or the resolution of any element into or its creation from primordial matter.

The case for the evolution of the elements from primordial matter or protyle has been very ably summed up by Crookes in his addresses to the British Association[1] and to the Chemical Society[2] and the arguments from analogy are presented in a most powerful manner, while in addition he has brought forward experimental proof of the possible existence of bodies (like praseo- and neodymium, the series of elemental bodies made known to us by Krüss and Nilson, and the numerous separate bodies into which it is probable that yttrium, erbium, samarium and other "elements" commonly so called, have been and are being split up) which, though neither compounds nor mixtures, are not elements in the strictest sense of the word. These bodies, which he styles meta-elements, consist of different groups which shade off so imperceptibly, the one into the other, that it is impossible to erect a definite boundary between any two adjacent bodies and to say that the body on this side of the line is an element, whilst the one on the other side is non-elementary, or merely something which simulates or approximates to an element. Yet by means of fractiona-

[1] Chem. News, 54, *115*; 1886. [2] Chem. News, 57, *198*; 1888.

tion these bodies may be separated one from the other and then they exhibit slight spectral differences.

Nordenskiold[1] has also shown that gadolina, which is a crude mixture of the yttrium group that may be precipitated from the minerals containing these earths, has a constant atomic weight and hence that these elements always occur in nature in the same proportion. If this be true for one it may be true for other cases and thus the idea of an element as something definite, primary and ultimate seems to be growing less and less distinct.

Finally, Grünwald has announced that during a mathematical investigation of the changes which the properties and especially the spectra of two bodies undergo when they unite to form a new substance, he has discovered a simple and important proposition of a future chemico-mathematical theory of perturbations, and by its means has shown the compound nature of hydrogen and oxygen and has demonstrated the dissociation of hydrogen in the sun. The method employed is a spectral one and requires conditions which cannot be reproduced at the will of man, so that if it stands the tests of criticism, which is doubtful, it will not then enable us to witness the evolutionary process in actual operation. Hence, we find for the doctrine of evolution in the domain of chemistry that the tests yield absolute results only when applied to compound matter, and that the extension of the doctrine to the genesis of the elements is a pure speculation which bids fair at present to be as incapable of absolute proof as is the nebular hypothesis.

[1] Compt. rend., 103, Nov. 2, 1886.

PAPERS READ.

COËFFICIENTS OF VOLATILITY FOR AQUEOUS CHLORHYDRIC ACID. By Prof. ROBERT B. WARDER, Washington, D. C.

[ABSTRACT.[1]]

AQUEOUS chlorhydric acid may be represented by the formula $H_2O + nHCl$, where n is the number of molecules of the acid, divided by the number of molecules of water. If the composition of the distillate formed at any moment (under certain experimental conditions) is $H_2O + vn\,HCl$, the factor v expresses the "coëfficient of volatility."

When dilute acid ($n = .07$ to $.11$) was subjected to rapid fractional distillation, the experiment agreed with the equation

$$v = 445n^2.$$

With slower boiling, the values of v were reduced from ten to thirty per cent.

When strong acid ($n = .15$ to $.20$) was boiled rapidly, the values of v were approximately represented by the equation

$$v = 3036n^4.$$

The results are illustrated with diagrams, and are discussed with reference to the thermal problems involved.

ON A NEW METHOD FOR THE DETERMINATION OF THE ATOMIC WEIGHT OF OXYGEN. By Prof. W. A. NOYES, Rose Polytechnic Institute, Terre Haute, Ind.

[ABSTRACT.]

THE apparatus which it is proposed to use consists of a U-tube filled with copper oxide, to one side of which is attached a tube with a capacity of about 20 cc. and to the other side a three-way stop-cock. The U-tube will contain 80 to 100 grams of copper oxide. It is made of hard glass and can be heated while the rest of the apparatus remains cold. In using

[1] This paper may be expected in full in the Annual Bulletin of the U. S. Geol. Survey for the chemical and physical division.

the apparatus, it is heated, exhausted by means of a Sprengel air-pump, cooled and weighed. The portion containing the copper oxide is then heated in an appropriate air-bath and a gram or more of hydrogen is passed in. The hydrogen is converted into water by the copper oxide and the water formed condenses in the side-tube referred to above. The apparatus is then cooled and weighed. After weighing, any gases which may remain in the apparatus are pumped out and analyzed, and thus a correction which may be necessary for impurities in the hydrogen is obtained. The apparatus is heated at the same time and all of the water formed is expelled. By weighing again, the amount of oxygen taken from the apparatus is determined.

The advantages of the method are: the weight of the hydrogen is found directly as the difference of two weights subject only to a small correction for impurities; the weight of the oxygen is also found directly and independently of the weight of the hydrogen; each is weighed in a vacuum, and by using a proper counterpoise any correction for the buoyancy of the air becomes unnecessary; impurities in the hydrogen and especially any nitrogen which it may contain will be detected; finally, there can be no error from incomplete combustion of the hydrogen.

HAMPE'S METHOD OF DETERMINING Cu_2O IN METALLIC COPPER. By Prof. FRED. P. DEWEY, Smithsonian Institution, Washington, D. C.

[ABSTRACT.[1]]

THE author found that Hampe's reaction $3Cu_2O + 6\,AgNO_3 + 3H_2O = H_6Cu_4N_2O_{12} + 2CuN_2O_6 + 6Ag$, whereby two-thirds of the copper is converted into an insoluble nitrate and remains with the Ag precipitated on treating pure Cu_2O, with $AgNO_3$ in neutral solution, is modified by the presence of Cu as follows:

$$4Cu_2O + Cu + 6AgNO_3 + 6H_2O = 2H_6Cu_4N_2O_{12} + CuN_2O_4 + 6Ag.$$

That is, all the Cu_2O present in the metallic copper remains in the insoluble material.

ON THE CONSTITUENTS OF WINTERGREEN LEAVES. (GAULTHERIA PROCUMBENS, LIN.) By Prof. FREDERICK B. POWER and NORBERT C. WERBKE, Madison, Wis.

[ABSTRACT.]

IN this investigation the authors have isolated the hydrocarbon or terpene from the volatile oil of wintergreen, which was stated many years ago by Cahours (Ann. Chim. Phys. (3), 10, p. 358) to be contained therein,

[1] Published in full in Proc. U. S. National Museum for 1888.

and have confirmed and determined some of its physical properties. They find the terpene to be present in very much smaller amounts than has been previously stated and generally accepted. The distinguishing characteristics of the oil of wintergreen and the closely related volatile oil of sweet birchbark (*Betula lenta*, Lin.) are also demonstrated.

Experiments were furthermore made to ascertain whether the crystalline poisonous principle, isolated by Professor Plugge, of Groningen in the Netherlands, from the leaves of several ericaceous plants, and termed by him *andromedotoxin*, is also contained in the leaves of the Gaultheria or wintergreen. This is proved not to be the case.

ON THE OXIDATION OF NITRO-P-XYLENE WITH POTASSIUM FERRICYANIDE

By Prof. W. A. NOYES, Rose Polytechnic Institute, Terre Haute, Ind.

[ABSTRACT.]

A SERIES of experiments has been conducted for the purpose of discovering any general laws which may govern the oxidation of benzene derivatives by potassium ferricyanide. One of the most interesting results of the work has been the discovery, by Moses and myself,[1] that meta-nitrotoluene is oxidized with much greater difficulty than ortho-or para-nitrotoluene. This rendered it probable that if mono-nitro-para-xylene were subjected to the action of the agent in question, it would yield β-nitro para-toluic acid $\left(C_6H_3 \begin{matrix} \diagup CO_2H & 1. \\ -NO_2 & 2. \\ \diagdown CH_3 & 4. \end{matrix}\right)$, a substance unknown at the time this investigation was begun. On carrying out the oxidation, the acid was obtained, as expected, but only in small quantities. Larger amounts of the acid were obtained by starting with nitro-para-toluidine and converting it by means of Sandmeyer's reaction. This method has recently been used independently by St. Niementowski.[2]

Barium, calcium and copper salts of both the nitro and the amido acid were prepared and analyzed.

A second product of the oxidation was a substance having the composition of nitro-tere-phthalic acid with the addition of two molecules of water. As this substance could not be obtained by crystallizing nitro-terephthalic acid under a variety of conditions, and was only formed by the oxidation of nitro-xylene by potassium ferricyanide, the water is probably water of constitution and not water of crystallization. The substance probably has the formula, $C_6H_3 \begin{matrix} \diagup C(OH)_3 \\ -NO_2 \\ \diagdown C(OH)_3 \end{matrix}$, and its formation is analogous to the formation of ortho-sulphamine acids where sulphinides would be expected.[3]

[1] Am. Chem. Journal, 7, 149. [2] Ber. d. Chem. Ges., *21*; 1992.
[3] Am. Chem. Journal, *8*, 179.

PROPYLIDENE DI-ETHYL AND DI-METHYL ETHERS. By Prof. SPENCER B. NEWBURY, Cornell University, Ithaca, N. Y.

[ABSTRACT.[1]]

BY the action of ethyl alcohol and methyl alcohol on propionic aldehyde in presence of glacial acetic acid, propylidene di-ethyl ether and propylidene di-methyl ether were obtained, the former the next higher homologue of ordinary acetal. The reactions which took place are as follows:—

$CH_3—CH_2—CHO + 2C_2H_5OH = CH_3—CH_2—CH(OC_2H_5)_2 + H_2O.$

$CH_3—CH_2—CHO + 2CH_3OH = CH_3—CH_2—CH(OCH_3)_2 + H_2O.$

Propylidene di-ethyl ether boils at 122°C.; spec. grav., 0.8478 at 0°.

Propylidene di-methyl ether boils at 86°–88°C.; spec. grav., 0.8657 at 0°.

A NEW VEGETABLE DYE. By Dr. THOMAS TAYLOR, U. S. Department Agriculture, Washington, D. C.

[ABSTRACT.]

A RICH crimson liquid extracted from a mushroom of the genus *Lactarius*, when applied to paper produces a delicate salmon tint. An alkali applied to the paper thus stained changes the tint to lilac, thus affording a test for alkalies. The lilac color changes again to salmon on the application of a weak acid.

FATAL POISONING BY CARBON MONOXIDE. By Prof. W. P. MASON, Troy, N. Y.

[ABSTRACT.]

THREE deaths, and a number of serious illnesses, were caused by the breaking of a "fuel gas" main, and the consequent escape into adjoining houses of an odorless gas containing 37.5 per cent carbon monoxide. A specimen of blood which was taken from the heart cavity of one of the victims, although twenty months had elapsed since it was collected, still presented the same brilliant red color, and gave the spectrum absorption bands as strongly as when fresh.

THE SAFETY OF COMMERCIAL KEROSENE OILS. By Prof. SPENCER B. NEWBURY, Cornell University, Ithaca, N. Y.

[ABSTRACT.[1]]

TESTS made by the author show that the oil in modern lamps with circular or double wick often becomes heated to 110° to 112° F., a tempera-

[1] The full text of this paper will appear in the American Chemical Journal.

ture considerably above the usual legal flashing point. As, however, it is not yet proved that the vapor of oils of high flashing point is capable of giving dangerous explosions with air at any temperature, experiments were made in the hope of establishing a clear relation between the flashing point and the danger of explosion.

The following conclusions were reached:

1. All the paraffines up to decane, and probably also higher members of the series, will at suitable temperatures form explosive mixtures with air.

2. Oils which flash at a point considerably above 100° F. may under proper circumstances give violent explosions at their flashing temperature.

3. An oil consisting of pure decane, $C_{10}H_{22}$, would be accounted a safe oil by the legal flashing point test of New York state, while one consisting of nonane, C_9H_{20}, would be below the standard.

A sample of illuminating oil of good quality yielded on fractional distillation about one and one-half per cent of octane, and notable quantities of nonane and decane. To samples of the same oil, and also to samples of "mineral sperm" oil, varying proportions of pentane, hexane and heptane were added, and the reduction of the flashing points noted.

These experiments show that an oil heated above its flashing point is dangerous, whether that point be high or low. Oils which flash below 120° F. are not safe to use in the powerfully-heating lamps which are now so largely employed.

Lake Erie water at Cleveland, Ohio. By Albert W. Smith, Cleveland, Ohio.

[ABSTRACT.]

An account of a chemical examination of the water with reference to locating the new tunnel of the Water Works' Department.

Some notes on progress in chemical methods of water analysis with especial reference to the ammonia process. By Prof. Frank H. Morgan, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

The lines of enquiry which now demand the attention of workers in this direction may be summarized as follows:

First. A careful revision and adoption of uniform methods of procedure for those processes at present in general use.

Second. Careful investigation of modifications of the "ammonia" and

"oxygen" processes, designed to render more definite results as to the nature and amount of organic matter in a water.

Third. Continued studies of the conditions of nitrification in the soil and its relations to purification of both well and river waters.

Fourth. Trials of new reagents in a search for means of distinguishing different kinds or conditions of organic matter.[1]

Fifth. A careful study of the relations of time, temperature, concentration and pressure to the action of reagents on the organic matter.

Sixth. A complete index to the literature of potable water analysis and carefully drawn monographs of each method in general use.[2]

The following lines of investigation are being carried on by the writer:

1. Trial of the action of a constant volume of concentrated alkaline permanganate.

Results so far obtained show no decided increase in the amount of nitrogenous matter thus converted into ammonia.

2. Trial of the amount of ammonia that may be obtained by a preliminary treatment with H_2SO_4. Application of the principles of Kjeldahl's process was suggested by Prof. E. Waller, Jour. Am. Ch. Soc., 8, 150, but no results have as yet been published.

Present progress of the writer shows the possibility of a considerable increase in the amount of ammonia as compared with the total ammonia obtained by the ordinary process, but satisfactory regularity has not yet been secured.

3. Determination of the amount of ammonia developed by the action of Na_2CO_3 on the organic matter during the ordinary distillation of free ammonia.

The method used is to determine the real saline or free ammonia by direct test and subtract this from that obtained by the ordinary distillation with Na_2CO_3.

The following results have been obtained:

	Ammonia in p. p. m.		
	Direct.	Dist.	Diff.
Diluted fresh sewage	9.62	12.45	2.83
Same sewage forty-eight hours older	16.80	18.20	1.40
A well water from a suspected district (alb. am. 0.10 and nitrates n = 5. p. p. m)	.016	.023	.007

Many experiments have been made upon the details of the ammonia process. A small amount of CO_2 in a distillate may diminish the color developed by Nessler reagent. The amount of Nessler to be added to the

[1] Practically only one reagent has been in general use.

[2] The writer is making an index to the "Ammonia Process" and hopes to present a monograph at the next meeting.

first distillate from a bi-carbonated water should not be less than 2.5 cc. for 50 cc. of distillate.

In conclusion, the adoption of a regular method of procedure in the use of the ammonia process is urged upon the association to secure uniform and comparable results from different workers. The adoption of the methods of stating results of water analysis reported last year will be of little advantage unless followed by such a revision and adoption of methods of procedure as shall render the results definite; the work of different chemists being comparable only when based upon constant and uniform conditions.

PRELIMINARY NOTE UPON IODINE AS A REAGENT IN THE ANALYSIS OF DRINKING WATER. By Prof. FRANK H. MORGAN, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

A DILUTE solution $\left(\frac{n}{500}\right)$ of iodine, in presence of sulphuric acid, acts in a significant manner, bearing a definite relation to the character of the water treated.

While permanganate of potash in acid solution acts on nearly all classes of organic bodies, iodine in acid solution has a more limited and specific action.

The procedure, which has given satisfactory results, consists mainly in adding a measured amount of iodine solution to 100 cc. of the water or solution to be treated, acidifying with sulphuric acid, and subjecting to any desired temperature (generally 100° C.) in hermetically sealed flasks. After a definite time the flask is cooled, opened, and the iodine absorbed determined by titrating the free iodine by thiosulphate.

A sewage, which by the permanganate process

consumed $KMnO_4$	equivalent	to 28.7	p.p.	m.	of	oxygen.
absorbed iodine	"	" 7.4	" "	"	"	"
A river water consumed $KMnO_4$	"	" 8.8	" "	"	"	"
absorbed iodine	"	" 0.87	" "	"	"	"

SOME MODIFICATIONS OF THE METHODS OF ORGANIC ANALYSIS BY COMBUSTION. By Prof. WM. L. DUDLEY, Vanderbilt University, Nashville, Tenn.

[ABSTRACT.[1]]

SOLID substances are burned in a porcelain or platinum boat contained in a platinum or glass tube which is open at both ends. One end is connected with the calcium chloride tube and potash bulbs in the usual manner, and the other with apparatus to purify and dry the air and oxygen which are passed through the tube during the combustion. The tube is

[1] The full paper will be printed in the American Chemical Journal.

filled one-third full of granulated oxide of manganese prepared by strongly heating the nitrate over a blast lamp. The boat containing the substance is introduced, the collecting bulbs and tube are attached, and a current of air is passed through the apparatus. The burners are lighted under both ends of the tube and the substance is approached in the usual way. The combustion is finished in a current of oxygen.

Non-volatile liquids are placed in a boat and treated in substantially the same manner as solids.

Volatile liquids are weighed in a bulb having two tubes (each opposite the other) which are attached by rubber tubing to and between the gas purifying apparatus and the combustion tube which is filled about two-thirds full of oxide of manganese. The combustion tube is heated to redness, and a current of dry nitrogen is passed through the apparatus to act as a carrier for the volatile liquid which is slightly warmed. When all of the liquid has passed into the tube a current of oxygen is turned on to complete the combustion.

PRELIMINARY NOTE ON THE FINAL PRODUCT OF THE ACTION OF CONCENTRATED SULPHURIC ACID ON SUGAR. By Prof. FRANK H. MORGAN, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

IN some experiments on the solution of carbon by the action of concentrated sulphuric acid and potassium chlorate, the writer had occasion to analyze various products of the action of sugar and sulphuric acid; the composition of these products led him to treat sugar with boiling concentrated sulphuric acid to see how nearly pure carbon could be obtained in this way. The final product obtained, upon which the acid had no more action, has an ultimate composition almost identical with Brodie's graphic acid upon which he based his determination of the atomic weight of "graphon".

Ultimate product of H_2SO_4 on sugar		Brodie's Graphic Acid $[C_{11}H_4O_5]$
Carbon	61.41	61.04
Hydrogen	1.77	1.85
Oxygen	36.82	37.11
	100.00	100.00

Further work is necessary to prove the constancy of this result.

THE CHEMISTRY OF FISH. By Prof. W. O. ATWATER, Middletown, Conn.

[ABSTRACT.]

THIS paper gives results of comparisons of analyses of American and European fishes and classifications by chemical composition.

THE QUANTITIES OF NITROGEN IN PROTEIN COMPOUNDS. By Prof. W. O. ATWATER, Middletown, Conn.

[ABSTRACT.]

THIS paper summarizes results of determinations of nitrogen in animal tissues and discusses the nitrogen-factor of protein.

ON THE PRESENCE AND SIGNIFICANCE OF AMMONIA IN POTABLE WATERS. By Prof. E. H. S. BAILEY, Lawrence, Kan.

COMPOSITION OF SALT BRINES IN NORTHERN OHIO WITH SPECIAL REFERENCE TO ITS BROMINE AND LITHIUM CONTENTS. By Prof. C. F. MABERY and HERBERT H. DOW, Cleveland, Ohio.

THE CRYSTALS OF BUTTER AND FATS, ILLUSTRATED BY LANTERN PICTURES. By Dr. THOMAS TAYLOR, U. S. Dept. of Agriculture, Washington.

EXHIBITION OF SPECIMENS OF ALLOYS OF CAST IRON AND ALUMINUM. By W. J. KEEP, Detroit, Mich. (By request of the Section.)

THE PIERCE PROCESS OF MAKING CHARCOAL, AND EXHIBITION OF SPECIMENS OF PRODUCTS. By Prof. O. H. LANDRETH, Nashville, Tenn. (By request of the Section.)

PRESENTATION AND DISCUSSION OF LABORATORY METHODS AND DEVICES By MEMBERS OF THE SECTION.

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PAPERS READ.

THE INFLUENCE OF ALUMINUM UPON CAST IRON. By W. J. KEEP, C. E., Prof. C. F. MABERY and L. D. VORCE of Cleveland. Read by W. J. KEEP, Detroit, Mich.

ALUMINUM is a metal obtained from its oxide, alumina. It is white in color and very tenacious, and it alloys readily with iron.

Cast iron, ordinarily used, is iron which contains all the carbon that it could absorb during its reduction in the blast furnace.

This carbon, when found in chemical union with the iron, is called combined carbon. In this state it cannot be seen. It is also found mechanically mixed with the iron in the form of graphitic carbon, when it becomes visible.

Other elements commonly found in cast iron are phosphorus, sulphur, manganese and silicon.

The natural condition of carbon in iron is the combined state. The presence of silicon drives a portion of the carbon into the graphitic state.

Sulphur, manganese and phosphorus do not cause the carbon to leave its natural combined state, and if silicon be present, these elements either drive it out or overpower it.

Carbon is therefore a passive element and is made to change its form by the presence of other elements. It is this change of carbon which indicates, to the eye, the influence of any element upon the cast iron.

Iron and combined carbon, or carburetted iron, is called white iron, and the grain is generally very fine, and often even, and the metal is very hard.

Graphite darkens the fracture until it becomes a very dark gray, and the grain is coarse and irregular. With increase of graphite the metal becomes soft.

We shall confine ourselves in this paper to the influence of aluminum upon cast iron.

Let us for a moment review the present knowledge on this subject. It is known that fused wrought iron, a mixture of cast iron and steel, or steel alone, either of which would make castings which would be full of blow holes, will make solid and homogeneous castings if as small a quantity of aluminum as one-tenth of one per cent is added just before pouring. Also, that such addition causes the iron to remain fluid long enough to allow its being cast into moulds.

It seems to be the general opinion that the aluminum does not remain in the metal, but that it exerts its influence between the time of its introduction and the time of its departure.

This seems to be the sum total of the present information regarding the influence of aluminum upon iron.

We propose in this paper to give the results of a series of very carefully conducted tests to substantiate further the statements just made, and to settle the question as to whether aluminum remains in the casting; also to determine the influence of this metal upon the physical structure and upon the composition of iron.

The physical tests that we have employed are what are known as "Keep's tests;" and by them we are enabled to make apparent to the eye the influence of any element upon cast iron.

When it was understood that we were to undertake this examination, the Cowles Electric Smelting and Aluminum Company of this city (Cleveland) kindly furnished us with what ferro-aluminum we needed, and Prof. C. F. Mabery and L. D. Vorce, of the Case School of Applied Sciences, of this city, volunteered to undertake the chemical examination of the test bars. The results of these investigations will be appreciated, when it is understood that we began without the least expectation of the very important results we have obtained, and that the methods for the determination of minute quantities of aluminum were so imperfect, that the small quantities used in the "Mitis" process could not be determined, if they still remained in the castings.

Regarding the physical tests, we should state that we use two bases: one, a white iron, with composition Si .186, P .263, S .0307, Mn .092; the other, a gray Swedish iron marked FLM, with composition, Si 1.249, P .084, S .04, Mn .187. The ferro-aluminum contained Si 3.86, Al 11.42.

The melting was done in a covered plumbago crucible, in a coke furnace driven by a blast of two and one-fourth ounces. The test bars were one foot long and cast in pairs; one, one-half an inch square and its mate one-tenth of an inch thick and one inch wide.

We started with thirty pounds of the base in the crucible; at the first heat there were cast four pairs of bars from the base alone, which took five pounds of metal. After allowing the remaining metal to become solid, we returned the runners of the first cast, and added four pounds of the base, and returned the crucible to the furnace. When nearly melted, we added enough ferro-aluminum to bring the percentage of aluminum in the whole to where we wished it for the second set of bars. We proceeded in like manner through the entire series of heats. To arrive at the influence of the aluminum, we made another series of heats, with the same base with exactly the same conditions, only we did not add the aluminum. The difference between the two series of tests gives the effect of the aluminum.

We shall consider this subject under the following heads:

1. The solidity of castings and the prevention of blow holes.

2. Does the aluminum remain in the iron to exert an influence when the iron is remelted?

3. The effect of aluminum upon the grain, or the changing of the carbon from the combined to the graphitic state.

4. Taking away the tendency to chill.
5. The prevention of sand scale.
6. The effect upon hardness.
7. The resistance to a load gradually applied, or a dead weight.
8. The resistance to a load suddenly applied, or impact.
9. The elasticity.
10. Permanent set.
11. The effect on the shrinkage of the iron.
12. The fluidity of the melted metal.

(1) *The solidity of castings and the prevention of blow holes.*

All of our tests bear upon this subject, but we have made one test, using the white base iron and one-tenth of one per cent of aluminum. It is almost impossible to get a solid casting of the white base alone, and its resistance to weight is generally about 175 pounds for the one-half inch square bars, and its resistance to impact is about 100 pounds. We have obtained, however, exceptionally sound castings of this base, and we shall use the strength of such castings for comparison.

These sound castings of the white base alone resisted a weight of 379 pounds. With one-tenth of one per cent of aluminum added it resisted 545 pounds, a gain of 166 pounds, or about forty-four per cent from this small addition.

Measuring the resistance to impact, the white alone was 239 pounds; with aluminum 254 pounds, or about six per cent gain.

The castings appear of slightly finer grain, and the character of the crystallization is somewhat different, but the secret of the strength lies in the closing of spaces between the grains, or in other words, in the increased solidity of the casting. No other change is noticeable in the metal.

A graphic representation of this test is not needed.

(2) *Does the aluminum remain in the iron to exert an influence when the iron is remelted?*

CHART 1.

To determine this, we made a series of six heats from the white base, and added to the first heat one-fourth of one per cent of aluminum. This amount alters the grain very perceptibly, making it whiter and finer, and removing the tendency of the base to a slight specular appearance, and giving a homogeneous fracture. It increases the strength above the base about twenty per cent to resist weight, and for impact, an increase of over seventy per cent.

The next heat was a remelt of the first, with the runners of the first cast put back, and enough white base added to reduce the aluminum to two-tenths of one per cent when the second cast was made.

Our comparisons will now be made between this series and the comparison series of the base alone. Looking at the chart we see that the effect of the aluminum in this second heat is greater than it was in the first heat to which heat the aluminum was added. This is due to the increasing porosity at each heat of the base when melted alone, and to the solidity of

the series with aluminum. At the third and subsequent heats the same result is apparent, the remaining aluminum causing more solid castings, though the continued additions of white iron at each heat and the consequent lessening of aluminum render the castings less strong at each remelting. Yet the effect of the aluminum is so constantly apparent at each melt, as to leave no doubt as to its presence even in the sixth remelting.

The chart, which we have prepared, shows these effects both as to weight and impact.

As we proceed with the description of other tests it will be noticed that we add but a small quantity of aluminum at each heat, and depend upon the additions made at previous heats to bring up the required percentage.

The results of the tests show conclusively that the aluminum remains and exerts its influence in subsequent casts as fully as would be expected.

(3) *The effect of the aluminum upon the grain, or the changing of carbon from the combined to the graphitic state.*

Let us say a few words in regard to the way in which, and the reason why, carbon takes on the graphitic form.

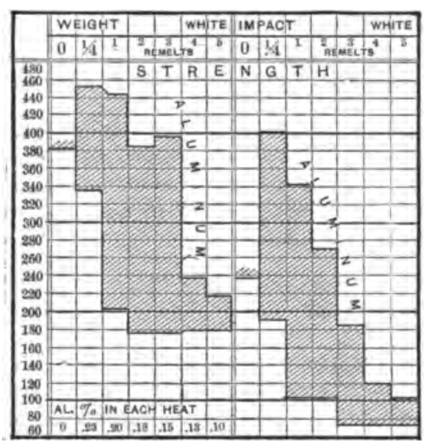

CHART 1.

All of the carbon, both combined and graphitic, which the iron is capable of holding when solid, must be dissolved and exist as combined carbon in the melted iron. Cast iron made in the usual way contains all of the carbon that it can hold.

Very often cast iron, when melted, contains more carbon than it can hold in combination when at a lower temperature; if so, as the iron cools down, such excess of carbon will separate as graphite and rise to the surface.

In any case, when a melted iron contains more carbon than the iron can hold in combination when cold, all of the excess will not be able to reach the surface, though it may not be visible to the eye, in the casting.

The introduction of other elements into the melted metal may alter its ability to hold the carbon. Sulphur causes it to let some go, while manganese enables it to hold more carbon in solution. Silicon also somewhat diminishes the capacity of the molten metal to retain carbon while it is liquid.

Aluminum allows most of the carbon to retain its natural combined form until the metal is too thick for the separated carbon to escape, but at the

instant of solidifying aluminum causes the iron to drop a portion of its carbon from the combined state. This liberated carbon takes the graphitic form and is imprisoned in the otherwise solid iron.

The advantages arising from a change of carbon from the combined to the graphitic state, at the instant of crystallization, are that all of the carbon thus liberated is imprisoned uniformly throughout the casting, and is not accumulated in pockets forming soft and hollow spots, as would be the case if liberated while the casting was yet fluid. Aluminum more than any known element accomplishes this. It not only changes white iron to gray, but seems at once to change the whole character of the metal. The drop of carbon seems to be instantaneous, at the instant of crystallization, and for this reason the time taken in cooling has little effect.

In fact, when the aluminum obtains full control of the carbon, it would seem that the more sudden the cooling the more the formation of the graphite, and the thin portions of the casting are therefore as gray as the thicker portions.

The powerful and positive influence of aluminum upon the carbon, and therefore upon the grain and color of the iron, is shown by an examination of the series of samples that we present here to-day.

Take those made from the white iron base with almost no silicon present; the base alone gives a white bar full of blow holes. An addition of one-fourth of one per cent aluminum gives us not only a perfectly homogeneous and solid casting, but the color is darker and the grain shows that some of the carbon has taken the graphitic form.

The thin casting shows this even more than the heavier bar, showing that the change occurred suddenly and that time had but little effect.

Examining each bar in turn we see that each similar addition of aluminum produces a corresponding effect, until at the third addition, or with three-fourths of one per cent, the casting is gray with no sign of white either in the square or in the thin bar.

The set of tests with the gray iron base, containing one and one-fourth per cent of silicon, shows that silicon and aluminum work together in the same direction, and that a slight addition of aluminum takes the white out of the casting at once, giving the same grain in a thin as in a thick casting.

This effect increases as the aluminum increases, and the indications are that at least up to four per cent, the limit of our experiments, the more aluminum, the softer and grayer the castings.

(4) *The taking away the tendency to chill.*

CHART 2.

If cast iron be cooled very suddenly, the carbon, which the melted metal holds in combination, will not have time to separate and will be retained in the combined state. Such castings are called chilled castings.

Chill is caused by molten iron running against a body which rapidly withdraws its heat, causing it to retain its carbon in the combined form.

Back from the chill, where this instantaneous cooling could not exert its full effect, a portion of the carbon takes the graphitic form.

This property is made use of when it is desirable to obtain hard wearing surfaces, and in the same casting tough and soft central portions, as in car wheels.

While this chilling effect is exceedingly valuable for many purposes, yet generally speaking, the founder desires exactly the reverse.

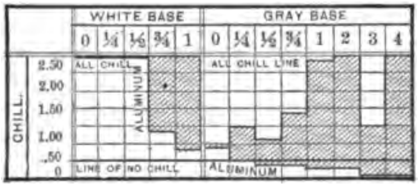

CHART 2.

Correction of Chart 2.—*Consider the 0 as belonging to the lower line of the cut, the .50 to the next line, and bring 1.00 down to the third line, 1.50 to the fourth, and so on. All these readings are inches.*

We have said that aluminum causes the carbon to assume the graphitic form on the instant of solidifying, and therefore the sudden abstraction of heat does not imprison the combined carbon and cause chill.

This effect of aluminum is to give a uniform grain for thick and thin castings, and not allow the coldness of the mould to affect the grain.

(5) *The thickness of the sand scale.*

This is an important consideration; for the sand must be cleaned from the casting, and the surface must first be cut before the interior can be reached.

To prevent the iron from burning the sand into itself and thus forming a scale, a plumbago facing is sifted on the surface of the mould, but it is difficult for the facing to lie on the surfaces or to resist the intense heat of the metal.

When aluminum in an iron causes the dropping of the graphite from the mass of the metal, that graphite which is on the surface of the casting separates and forms a perfect plumbago facing, which opposes the sand and the heat. It will therefore be seen that in castings having sufficient aluminum to cause this separation of graphite, there will be no sand clinging to the face, and that the surface will be as soft as the interior of the casting. Every iron worker will appreciate this good effect of aluminum.

(6) *The effect upon hardness.*

Hardness in cast iron is caused by the carburetted or white iron, in masses large enough to oppose the tool. If the carburetted iron exists in minute threads stretched around atoms of graphite, a tool will easily cut it and it will not be considered hard.

This graphitic carbon, minutely dividing the mass, gives the tools of the workman a chance to cut or break the films of metal, giving what we call softness to the iron. The later the carbon is dropped, the smaller will be the atoms of graphite and the closer the grain. Yet this greater subdivision will, for the reason just given, make the iron work more easily.

The fineness of the grain of iron affected by aluminum causes such iron to be much more easily cut than iron of coarser grain.

The next question to consider is that of strength.

The power of wrought iron and steel to resist extension is so great that where such stresses are to be resisted, decarbonized metal should be used. The resistance of any cast iron to crushing is so great that we need not consider this.

The forces which cast iron structures should be made to resist, aside from crushing, are a dead weight or a blow applied transversely. We should therefore test cast iron with these forces.

(7) *The resistance to a load gradually applied, or a dead weight.*

CHART 3.

If we compare the transverse breaking weights of the two series which we have been considering, number by number, we perceive that the aluminum has increased the strength to sustain a constant load.

This is a very important effect and perhaps comes partially from the tenacity and strength of aluminum itself, but probably more from the uniform grain of the iron.

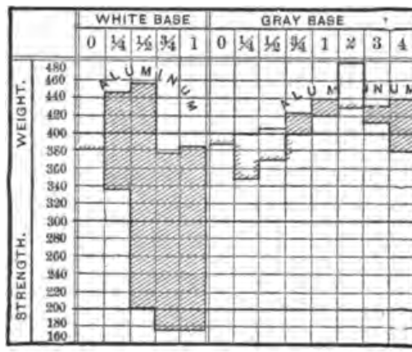

CHART 3.

(8) *The resistance to a load suddenly applied, or impact.*

It may be thought that the effect is substantially the same, whether the force be a constant weight or a suddenly applied blow. We shall at a future time prove that the effects are not the same and that an iron should be tested by a blow if it is expected to resist impact.

CHART 4.

By a comparison of the graphic representation, we see that the capacity to resist impact is increased by the addition of aluminum much more than the capacity to resist a dead weight. It will be seen at a glance that the test bars made with the white base are benefited far more than those made with the gray base. The reason for this is, that the white base alone made porous castings; at each remelt this porosity increased, due to the continuation of the heat, running the strength down to 68 pounds at the fifth heat.

The first, and each subsequent addition of aluminum, caused the castings to be perfectly sound, and the infinitesimal atoms of graphite deposited throughout the metal removed the rigidity and brittleness of the initial metal.

The gray iron base contained enough silicon to accomplish all this, and the only effect on strength that the action of the aluminum on carbon could have, would be to increase the fineness of the grain unless the toughness of the aluminum itself could give strength to the casting, though the aluminum no doubt removed any slight blow holes that existed in the initial gray metal.

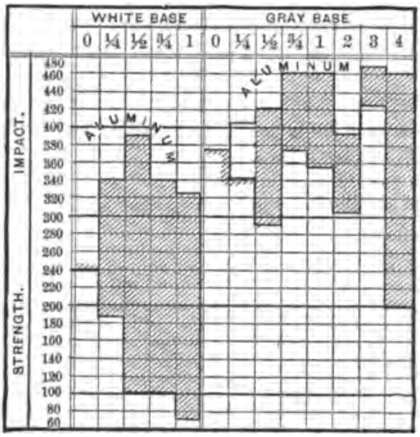

CHART 4.

This leads us to notice that each addition of aluminum increases the strength over that of the initial metal. We must expect, however, that after we have added enough aluminum to cause a solid casting, and to remove the brittleness, that the dividing up of the mass by the atoms of graphite accomplishes, any further additions of aluminum and consequent increase of graphite (which has no strength of itself) must weaken the casting.

(9) *The elasticity.*

CHART 5.

The compactness and closeness of the grain of cast iron when aluminum was the agent by which the graphite was precipitated, and the fine attenuation of the veins of the carburetted iron, cause the metal to be very elastic and, as we have seen, not so brittle as without aluminum.

(10) *Permanent set.*

This is caused by the compression of the graphite within the framework of carburetted iron. When this compression of graphite carbon is produced by transverse bending, the framework of the metal also takes on a permanent form which cannot be altered except by a greater force than was before applied.

The fineness and compactness of iron alloyed with aluminum gives less permanent set than iron equally as soft, when such softness is produced by silicon.

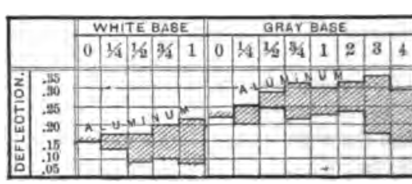

CHART 5.

(11) *The effect on the shrinkage of the iron.*

The more suddenly and completely the carbon is changed from combined to graphitic, at the instant of crystallization, the more space will the casting occupy. When the casting is cold, it will therefore have contracted

less than if more carbon had remained combined. White iron, having most of its carbon in the combined state, shrinks from one-fourth to one-third of an inch in each foot.

Gray iron sometimes shrinks as little as one-tenth of an inch to each linear foot. As the combined is the natural state for the carbon, we may say that this maximum shrinkage is the natural shrinkage for cast iron having its carbon combined. We can therefore say that aluminum takes out or reduces shrinkage when a sufficient quantity is added. This is a very great advantage, as shrinkage requires great skill in the preparation of patterns to prevent warping and cracking, and violent internal strains within the castings.

The lessening of shrinkage avoids these evils, and is therefore a great gain.

CHART 6.

If you will look at the chart for shrinkage, you will see the most conclusive proof of our explanation of the way in which shrinkage is lessened. With both the white and the gray bases, during the first two additions, the shrinkage of the square bar is slightly increased.

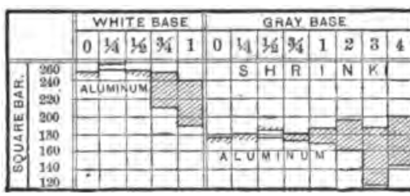

CHART 6.

The influence of the aluminum thus far has been in the direction of elimination of blow holes, and causing an even distribution of the dark and light grains.

At the third addition, however, when the amount reached three-fourths of one per cent, the effect was appreciably felt upon the carbon, as seen by the color, and as we should expect from the deposition of this large bulk of graphite, the casting does not shrink as much, and each addition of aluminum increasing this bulk of graphite decreases the shrinkage.

CHART 7.

The effect upon the grain and color of the thin bars of the series is very remarkable, showing that the aluminum has changed enough carbon to graphite to produce a dark, even grained casting.

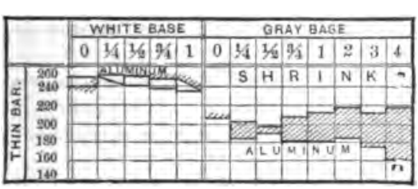

CHART 7.

The effect upon the shrinkage of these thin bars is as we should expect, and is more marked even than in the square bars.

The shrinkage in the thin bars of the white series shows a constant decrease as the aluminum increases, but in the series for comparison the shrinkage dropped still more rapidly.

If a new crucible was used in commencing this comparison series, enough silicon might have been absorbed to produce this effect. This leads us to remark that on account of the variations of conditions in any series of tests, that cannot be foreseen, we must avoid drawing any but general conclusions, and these should be based upon a large number of experiments.

(12) *The fluidity of the melted metal.*

CHART 8.

Our tests of fluidity are correct as far as each individual heat is concerned, but variation may be due to the heat of the metal of that particular cast when poured. Viewed in a general way, the indications are that with the white base, with almost no silicon, the aluminum has increased the fluidity; but judging from the series with the gray base we would say that combined with silicon aluminum reduced the fluidity.

Our remarks in connection with shrinkage show that a sharp casting is produced by the instantaneous dropping of graphite when crystallization takes place; and that if the iron is fluid enough to fill the mould, any extra fluidity causes the iron in shrinking to draw away from the mould.

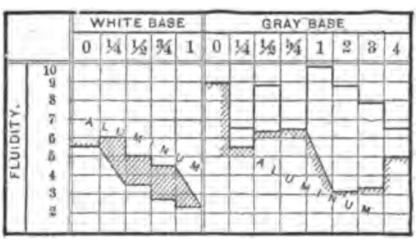

CHART 8.

Again, the percentage of aluminum necessary to bring about these desirable results will be too small to have much effect upon the fluidity of the metal.

The fact of the iron giving sharper and more perfect castings, on account of the swell of the casting, caused by the deposition of graphite at the instant of solidification, might cause the iron to be pronounced more fluid, if judged by the appearance of the castings.

No doubt the presence of varying quantities of manganese, sulphur, phosphorus and silicon, in the cast iron used, would modify the influence of aluminum, and until this is understood it may require considerable experiment to determine the amount of aluminum required, or how it shall be introduced.

This hurried presentation of the remarkable effects of aluminum upon cast iron will give an idea of the great benefit which is now promised to the iron founder by the rapidly falling price of aluminum as cheapened by the electric furnace.

We have already occupied all the time that was set apart for us.

Following the publication of this part of the subject, we shall soon present the results of the laboratory work of Professor Mabery and Mr. Vorce, which will throw still more light upon this interesting subject.

THE SECOND LAW OF THERMODYNAMICS. By Prof. R. H. THURSTON, Ithaca, N. Y.

[ABSTRACT.]

THIS paper represents the views of its author respecting the various statements of the second law of thermodynamics given by various authors of authority. It considers the nature of a law as defined by science; compares the statements of the law to be examined, as made by many writers, and shows that they are commonly statements of simple phenomena and not of laws; and, finally, gives what is thought to be a true expression of the law which should be given place as the second law of thermodynamics, and shows in what manner it has value and where it finds application in the theories of heat-transformation and in the application of those theories and the science of thermodynamics to the operation of the heat engines.

THE ECONOMICAL PRODUCTION OF CHARCOAL FOR BLAST FURNACE PURPOSES. By OLIN H. LANDRETH, Vanderbilt University, Nashville, Tenn.

[ABSTRACT.]

THE economical production of charcoal on a scale commensurate with blast furnace needs has not received that attention and study its importance demands. Of all the charcoal produced, ninety-four per cent is used for the production of pig iron, the remaining six per cent furnishing the supply for all domestic purpose, manufacturing and the production of all metals other than iron, so that improvements or processes in charcoal manufacture to be of broad application must be applicable to blast furnace plants which are necessarily extensive. Though the ratio of production of charcoal iron to the total iron produced is decreasing, owing to the enormous increase in the output of coke and anthracite iron, the absolute amount produced is annually on the increase, since there are many purposes for which charcoal iron is essential and cannot advantageously be displaced by coke iron. Among these are the manufacture of the finer varieties of steel, the higher grades of chilling iron, malleable castings, tin plate, wire rods and for mixing with coke iron, in which it is found that the addition of a small amount of charcoal iron raises the character of the mixture for many purposes out of proportion to the amount added, so that notwithstanding the fact that it costs from $2 to $5 per ton more to produce a ton of charcoal iron than coke iron its peculiar properties maintain the demand for it, and such demand would increase and the character of manufactures would thereby be raised could its production be cheapened to admit it for those purposes for which it is desirable, but from which its price now excludes it. Fuel represents nearly one-half the cost of char-

coal iron, and herein lies the greatest promise of reduction of cost, particularly as the methods of charcoal making at present in general use for iron making are extremely crude and uneconomical. The only exception to this which has been widely adopted is the Pierce process invented by Dr. H. M. Pierce of Detroit, Mich., now a resident of Nashville, Tenn., whose process was devised in 1876 and has since been undergoing continued improvements and developments until now (1888) nine plants under this process are in operation in the southern and western states which aggregate in capacity over 325,000 cords of wood annually carbonized, with plants under negotiation of over 250,000 cords annual capacity. By this system the by-products preserved from the destructive distillation of the wood more than meet the expenses of manufacture, leaving the charcoal produced a clear profit. In outline the process is as follows: the charring is effected in circular, flat top, brick kilns holding fifty cords of wood each. The wood is charred by the heat produced by gas burned in a brick furnace under the kiln into and through which the products of combustion pass. The gaseous products of dry distillation of the wood pass from the kiln to condensers, where the tarry and liquid products are condensed and the gas sent back to the kiln. Thus none of the charcoal produced is burned to carbonize other wood as in the common pits or ovens. The gas which elsewhere is wasted is here not only sufficient to effect the carbonizing of the wood but furnishes fuel for the boilers required about the works.

The wood used is as thoroughly seasoned as the conditions of maintaining a year's supply in advance, cost of storage room and interest on capital invested in stock render economical.

All the common varieties in the localities of the various plants are used. The proportions of the varieties used at Nashville are approximately:

Oak,	50 %
Cotton wood,	15
Hickory,	10
Gum, maple, birch and other varieties,	25
	100 %

If not thoroughly dry when placed in the kilns, the carbonization of the wood is automatically deferred, by the absorption of the heat in the evaporation of the sap and other moisture, until the seasoning process is complete. This seasoning commences at the top of the kilns and proceeds regularly downward, by a definite plane of seasoning. When this plane reaches the bottom and the seasoning is complete, which is indicated by a sudden change in the color of the escaping vapors, the process of charring begins at the top and proceeds downwards precisely like the seasoning process.

The watery vapors driven off during seasoning are not preserved but are allowed to escape through vents temporarily left open around the base of the kilns and through the top of the kiln-chimneys, which, during this stage, are disconnected from the suction main and left open at the top.

The time required for the several stages in the cycle of operations in producing a kiln of charcoal is as follows:

For charging one kiln with wood, . .	2 days.
" completing the seasoning of the wood,	1 "
" carbonizing the wood, ...	7 "
" cooling the charcoal, ...	6 "
" drawing the charcoal, ...	2 "
Total length of cycle, . .	18 days.

As one 60-ton blast furnace requires five thousand bushels of charcoal daily, or the output of two kilns, the total number of kilns in a plant to furnish a continual supply of fuel must be equal to twice the number of days in a cycle plus a margin for relays, for repairs, and unusual delays; the margin is usually chosen at one-sixth the effective number of kilns, so that the total number of kilns comprising a plant $= 2\ (18) + \frac{1}{6}\ (36) =$ 42, of which at any one time,

4	kilns are being charged and closed.
2	" " " seasoned.
14	" " " carbonized.
12	" " " cooled.
4	" " " drawn.
6	" " idle or acting as relays.
42	

These forty-two kilns are arranged in two distinct batteries of twenty-one kilns each. Each battery has its own condensers and suction main carrying the products of distillation to the condensers, and its own gas main leading the non-condensable gases back to the kiln furnaces.

The condensers are composed of tall wooden tanks five feet square by twenty feet high, through which the products of distillation pass, each enclosing ninety-nine vertical copper pipes two inches in diameter through which the condensing water flows. The condensed products are trapped out at the bottom of each condenser, of which ten comprise a battery, and conveyed to cooling tanks where the tar is separated from the pyroligneous acid liquor by cooling. The tar is not at present distilled further, but is used to coat the kilns to render them impervious to air, and for this purpose one coating of tar suffices for four burnings while the usual coating of lime whitewash has to be repeated after each burning. The circulation of the gaseous products through the system is maintained by exhaust fans which draw the non-condensed gases through the condensers and force them through the gas main back to the kilns when they are injected into the furnaces by a steam jet from a $\frac{1}{16}$-inch orifice playing in the center of a one inch nozzle on the gas pipe. The minimum amount of air necessary to effect the perfect combustion of the gases is admitted through regulating dampers in the front of the furnace.

From the liquor coolers the pyroligeneous acid liquor is conveyed to

the distilling house, where the acetic acid in the liquor is converted into acetate of lime; the liquor is then sent to the fractional distillation system which comprises eight primary stills and condensers, four intermediate stills and condensers and two final or shipping stills and condensers. The stills are circular tanks each holding about 2,500 gallons and are heated by steam coils of two-inch copper pipe. The several stills of each of the three series are operated abreast. The distillation is not carried on continuously, but each series is charged and the distillation carried on until all of the alcohol available is evaporated when the stills are emptied and recharged with new liquor. The degree of concentration attained in each series of stills is as follows:—

The liquor entering the primary stills contains	1½%	alcohol.
" distillate from " " " "	15%	"
" " " the intermediate " "	42%	"
" " " " final " "	82%	"

In the final form the crude alcohol is sent to the refineries. The resulting product, methylic alcohol or hydrate of methyl (CH_4O) is of wide application in the arts, being extensively used as a solvent for gums and varnishes for which purposes varnish manufacturers report it from four to five times as rapid in action as grain alcohol.

Methylic alcohol has never been produced by fermentation but, judging from its mode of production in the charcoal kilns, its artificial production by synthesis may be inferred as possible. As none of the charcoal is sacrificed for carrying on the carbonization of the wood and as the action of the kilns during carbonization and cooling is under the control of the operator, it is reasonable to expect a larger yield of charcoal per cord of wood than in the common methods of burning in ground pits or common bee-hive ovens. The resulting charcoal is also better adapted to bear the burden in the blast furnace than the common charcoal, being both firmer and denser as is shown by the fact that while the average weight of common charcoal per bushel in Tennessee does not exceed sixteen pounds that made from the same varieties of wood by this process weighs twenty pounds per bushel. The following exhibit shows the comparative amounts produced by the common and the improved methods respectively:—

The average yield of charcoal per cord of wood in U. S.	= 38.1 bu.[1]
" " weight per bushel of 2,688 cu.in. in U. S.	= 19.0 lbs.[1]
" " resulting ratio by weight of charcoal to wood in U. S.	= 18%
" " " " " " " " " " in France	= 19%[2]
" " " " " " " " " " in Belgium	= 16%[3]
" " " " " " " " " " in Pyrenees	= 17%[4]
The resulting general average in above countries	= 17.5%
The average ratio by weight, charcoal to wood, by Pierce process	= 25.8%[5]

[1] U. S. Forestry Report, 1882. [2] Valerius. Traite de la Fabrication de la Fonte.
[3] Sauvage. Ann. des Mines, 1837. [4] Francois. Traitment direct des minerais de Fer.
[5] Result of seven years practice at different plants.

The average ratio by weight, charcoal to wood, by Laboratory distillation, = 25.6%[1]

The resulting ratio of increase of Pierce process over average practice in the above mentioned countries = 44%

The resulting ratio of increase of Pierce process over average practice in U.S. = 83%

The quantitative values of the products resulting from the destructive distillation of the wood by this process are as follows, based on the proportion of the several varieties before given :—

DRY WOOD.	1.0 CORD.		4000 LBS.		100%
Resulting charcoal................	50.6 bus.	1012 lbs.		25.3 %	
" methylic alcohol........	4.4 gals.	30 lbs.		0.75%	
" acetic acid..............	4.6 gals.	40 lbs.		1.0 %	
" tarry compounds........	16.5 gals.	160 lbs.		4.0 %	
" water....................	220.7 gals.	1838 lbs.		45.95%	
" non-condensable gases..	11000.0 cu. ft.	920 lbs.		23.0 %	
Totals.........................	....	4000 lbs.	4000 lbs.	100%	100%

The non-condensable gases are in excess of the amount required to effect the carbonization of the wood and about one-third of all produced are used for fuel under the boilers which produce steam for heating the alcohol stills, pumping water for the condensers, running the pyroligneous liquor pumps, the exhaust fans for the non-condensable gases and the winding engines for the cable incline cars used in raising the wood from the wharf-boat to the kilns. This drawing off of a portion of the gas produced and its burning under boilers furnishes, together with the escape of kiln gases during seasoning, the necessary outlet to prevent the undue accumulation of the non-combustible gases arising from the combustion in the kiln furnaces and the carbonic dioxide driven off from the wood both of which would otherwise continually accumulate in the circulation system and ultimately choke the furnaces.

From the standpoint of industrial economics, the question of the economical production of charcoal in large quantities is one of importance, since the charcoal industry is extensive and the lines of possible improvement on the common practice are promising. These directions of improvement evidently consist:

1. In increasing the present yield of charcoal per cord of wood.
2. In improving the quality of the charcoal produced.
3. In utilizing to the best economy all of the available products of the

[1] Experiments on same varieties of wood as are used in the Pierce process conducted by Stolze, Smith, Lattimore, Pierce, Aszmus and Hessel.

destructive distillation of the wood of which the charcoal alone represents in value only about 35 per cent.

The total production of charcoal in the United States in 1887 was very approximately sixty-five million bushels; of this amount only about ten million bushels were produced by the process herein described and it is safe to assume that the production of all other improved processes combined did not make the aggregate of all charcoal produced by improved methods more than thirteen million bushels, or one-fifth the total production; on this assumption the following table has been prepared showing the returns to be expected from the general introduction of processes for charcoal manufacture which should equal in efficiency the results hereinbefore given. (See table, page 151.) This table shows not only the amounts of the several by-products annually preserved but also the annual saving in cords of wood carbonized and acres of forest land annually denuded if the present demands for charcoal were met by improved instead of unimproved processes.

This paper has been prepared independently of any effect its publication may have on the process herein described[1] and wholly for the purpose of showing by citing and describing one existing successful improved process that the common practice of charcoal burning is extremely wasteful, and that, instead of obtaining only charcoal and that moderate both in quantity and quality, it is possible to so effect the carbonization of the wood as that it shall not only furnish an increased yield of superior charcoal, but shall in addition supply the necessary fuel to haul itself to the kiln, to carbonize itself, to circulate and condense its gases and evaporate its liquors and shall furnish valuable chemical by-products whose proceeds more than pay the entire cost of all attendant operations and leave the charcoal as a clear profit to be divided between the producer and the consumer. A reference to the appended table shows that the general introduction of improved processes for the production of charcoal sufficient to meet the present demands would result in a saving of over four hundred thousand cords of wood per annum, a reduction of thirteen thousand five hundred acres in the amount of forest lands annually denuded or the liberation for other purposes of over four hundred thousand acres of land were the total supply of charcoal furnished from permanent timber preserves, and the preservation of by-products worth, at the lowest possible valuation, four million dollars per annum.

[1] The writer has no interest whatever either direct or indirect in the process herein described or in the publication of this paper which has been prepared without solicitation of any kind and solely from a belief that the account of the process might be of scientific interest.

TABLE SHOWING GAINS TO BE REALIZED BY THE GENERAL INTRODUCTION OF IMPROVED METHODS FOR THE MANUFACTURE OF CHARCOAL.

	On the basis of total production by unimproved methods.	On present basis of one-fifth production by improved and four-fifths by unimproved methods.	On the basis of total production by improved methods.	Present gain by use of improved methods.	Possible gain by use of improved methods for total production.
No. of cords of wood annually required to meet present demands for charcoal	1,706,000	1,624,800	1,300,000	81,200	406,000
No. of acres annually denuded to furnish above amount assuming 30 cords per acre	56,870	54,160	43,330	2,710	13,540
No. of acres required to be set apart for charcoal preserves assuming growth of wood to be one cord per acre per annum	1,706,000	1,624,800	1,300,000	81,200	406,000
No. of gallons of wood alcohol at three gallons per cord	None	780,000	3,900,000	780,000	3,900,000
No. of tons of acetate of lime at 150 lbs. per cord	None	19,500	97,500	19,500	97,500
No. of barrels of tar at ten gallons per cord	None	52,000	260,000	52,000	260,000
No. of million of cubic feet non-condensable combustible gas at 4000 feet per cord	None	1,040	5,200	1,040	5,200

ON THE INFLUENCE OF MOISTURE IN STEAM UPON THE STEAM CONSUMPTION PER LB. OF ENGINES OF LESS THAN FIFTY LBS. By. Prof. J. E. DENTON, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE paper describes an experiment with a 7 × 14 high speed engine in which water was injected into the steam pipe to the extent of about sixteen per cent of the total weight of steam used by the engine, and the resulting increase of steam consumption per lb. determined. It is shown that the increase in steam consumption was simply the water injected plus the latter's refrigerating effect in condensing steam with which it comes in contact.

The result indicates that the presence of moisture in steam does not induce a more uneconomical consumption of steam per lb. than is due to the amount of moisture itself; that is, does not increase the cylinder condensation of the engine, but further experiments are needed to establish a conclusion.

ON THE POSSIBILITY OF IDENTIFYING DRY OR SATURATED STEAM BY VISUAL OBSERVATION OF A JET OF SUCH STEAM FLOWING INTO THE ATMOSPHERE. By Prof. J. E. DENTON, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE paper describes an experiment in which a jet of steam flowing from a boiler at sixty pounds pressure into the atmosphere is superheated to a known degree, and then made to contain a known percentage of moisture by the abstraction of a known amount of heat from the jet.

Photographs were shown of the appearance of the jet of steam when the latter is dry, or slightly superheated, and when containing two and one-half per cent of moisture.

The conclusion reached is that as small an amount of moisture as one per cent causes a jet of steam to change in appearance to the naked eye so sensibly, that when a jet is perfectly transparent or invisible over a distance of about one inch from an orifice, which permits practically no wire drawing, it may be assumed that the moisture in the steam is less than one per cent.

RELATIVE ECONOMY OF HIGH SPEED ENGINES OF LESS THAN FIFTY LBS. USING STEAM BY EXPANSION AND THROTTLING RESPECTIVELY. By Prof. J. E. DENTON, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE paper gives the results of a series of experiments with a 7 × 14 high speed steam engine of the Buckeye type, whereby the steam consumption per horse power was determined for a large range of expansions at

variable cut-offs, and for a range of throttling at fixed cut-off. The experiments were conducted so that the same effective horse power was obtained at ninety pounds boiler pressure and a speed of 265–284 revolutions per minute: (1) By expansion without the use of a condenser; (2) By expansion and the use of a condenser to produce eighteen inches of vacuum; (3) By throttling without a condenser; and (4) By throttling with 18″ vacuum.

Diagrams and tables showing the results obtained were shown. The conclusions reached are:

1. That for ratios of expansion between one and one-half and seven, there is but little variation of steam consumption per horse power.

2. That the use of a condenser reduces the steam consumption of the engine per horse power for all practical expansions.

3. That "throttling" results in a considerable loss of economy.

EFFECT OF FRICTION AT THE CONNECTING ROD BEARINGS ON THE FORCES TRANSMITTED. By Prof. D. S. JACOBUS, Stevens Institute, Hoboken, New Jersey.

[ABSTRACT.]

LET P_w and P_c be the pressures of the wrist and crank pins upon the rod for frictionless pins; P_{wf} and P_{cf} the same when there is friction; r_w and r_c the radii of the wrist and crank pins; R the length of the crank; nR the length of the connecting rod and $\tan \varphi$ the coëfficient of friction.

Friction displaces the forces which the pins exert upon the rod, so that they are tangent to circles respectively equal to $r_w \sin \varphi$ and $r_c \sin \varphi$. These forces will exert moments upon the rod tending to rotate it and therefore affect all the other forces of the system, so that the forces P_w and P_c become P_{wf} and P_{cf} and are altered from their original line of action.

By a well-known principle in mechanics a force applied at any point is equivalent to an equal force in magnitude and direction at any other point plus a moment equal to the force multiplied by the perpendicular distance through which it has been displaced. We may therefore suppose the forces P_{wf} and P_{cf} to be applied at the centres of the pins if at the same time we introduce the moments $P_{wf}\, r_w \sin \varphi$ and $P_{cf}\, r_c \sin \varphi$.

For convenience we will suppose each moment to be produced by a pair of equal and opposite forces acting perpendicular to the rod at the centres of the pins; calling these forces A and B we shall have:

$$AnR = P_{wf}\, r_w \sin \varphi$$

and

$$BnR = P_{cf}\, r_c \sin \varphi$$

so that the moment introduced will be

$$AnR + BnR = (A+B)\, nR$$

Now instead of supposing P_{wf} and P_{cf} to act in a line tangent to the circles $r_w \sin \varphi$ and $r_c \sin \varphi$ no error will be involved in determining the

forces transmitted in the rod, if we suppose these forces to act at the centres of the pins, and introduce two additional forces A+B acting at the wrist pin perpendicular to the centre line of the rod and — (A+B) acting in an opposite direction at the crank pin.

The above reasoning may seem to involve a departure from the exact conditions of the problem, in which the forces are really applied in lines tangent to the circles $r_w \sin \varphi$ and $r_c \sin \varphi$, and it is the object of the paper to demonstrate that two such forces introduced into the solution for frictionless pins will give the correct results when friction is considered.

It is shown that P_{wf} and P_{cf}, as well as other forces such as the effort exerted on the crank, may be found with all desirable accuracy by employing the values of A and B found by the following approximate formula:

$$A = \frac{P_w r_w \sin \varphi}{nR} \text{ and } B = \frac{P_c r_c \sin \varphi}{nR}$$

For instance in the case of a 10″ × 12″ horizontal, high speed engine, with the extravagant assumption of 25 % coëfficient of friction, these formulae give A + B within ½ of one per cent. and P_{wf} and P_{cf} will be true within $\frac{1}{600}$ and $\frac{1}{1400}$ of one per. cent respectively.

General solution of the transmission of force in a steam engine, including the action of friction, acceleration and gravity.

By Prof. D. S. Jacobus, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

This problem has been discussed by various writers at home and abroad but among the numerous contributions to the subject the writer has found none which attempt a general solution, either some limited purpose being kept in view, such as the proper stiffness of the connecting rod, or the forces necessary to accelerate it, or else the problem is restricted in various ways, from fear of making it too complicated, as for instance by unnecessarily simple hypotheses and needless approximations. Further than this some of the solutions are faulty.

At the last meeting the writer contributed a special case of this problem for the purpose of giving the correct expressions for the accelerating forces in an ordinary steam engine, and discussing their practical application in various special cases. At that time the general solution, though essentially complete, was not in finished form for publication, the calculations of numerical results in several practical cases are yet to be completed which will still further delay the complete publication of the article for a short time.

In the general solution of the problem the following conditions must be assumed:

(*a*) that the centre of the crank shaft is not in the line of travel of the wrist pin.

(*b*) that the centre of gravity of the connecting rod is not in its line of centres.

(*c*) that the crank revolves at a uniform speed.

(*d*) that the moving parts have mass.

(*e*) that gravity acts at any angle with the engine.

(*f*) that there is friction between sliding surfaces.

The following formulæ include all of these conditions.

$$F_1 = m\tau^2 R\left\{\cos\theta + Z\right\}$$

$$Y_1 = -m\tau^2 R\left\{\left(\frac{n-}{n} + \frac{c(\sin\theta - b)}{n\sqrt{n^2-(\sin\theta-b)^2}}\right)\left(\frac{l}{n}\sin\theta + \frac{c}{n}Z\right) - \frac{\kappa^2}{n}\sin\theta\right\}$$

$$Y_2 = -m\tau^2 R\left\{\left(\frac{l}{n} - \frac{c(\sin\theta - b)}{n\sqrt{n^2-(\sin\theta-b)^2}}\right)\left(\frac{l}{n}\sin\theta + \frac{c}{n}Z\right) + \frac{\kappa^2}{n^2}\sin\theta\right\}$$

$$X_1 = m\tau^2 R\left\{\left(\frac{n-l}{n} - \frac{c\sqrt{n^2-(\sin\theta-b)^2}}{n(\sin\theta-b)}\right)\left(\cos\theta + \frac{n-l}{n}Z + \frac{c}{n}\sin\theta\right) + \frac{\kappa^2}{n^2}Z\right\}$$

$$X_2 = m\tau^2 R\left\{\left(\frac{l}{n} \mp \frac{c\sqrt{n^2-(\sin\theta-b)^2}}{n(\sin\theta-b)}\right)\left(\cos\theta + \frac{n-l}{n}Z + \frac{c}{n}\sin\theta\right) - \frac{\kappa^2}{n^2}Z\right\}$$

$$G_f = \frac{1}{1+\tan\varphi^1\tan\beta}\Big[\left\{P_a + C - F_1 - H^1 + D\sin\delta - (A+B)\sin\beta - X_1\right\}$$
$$\tan\beta + Y_1 + D\cos\delta - (A+B)\cos\beta\Big]$$

$$P_{wf} = \sqrt{(P_a + C - F_1 - H^1 - G_f\tan\varphi^1)^2 + G_f^2}$$

$$T_f = \left\{P_a + C - F_1 - H^1 - G_f\tan\varphi^1 + D\sin\delta - (A+B)\sin\beta - X_1\right\}$$
$$\sec\beta\sin(\theta+\beta) - Y_2\cos\theta - X_2\sin\theta - (A+B)\cos(\theta+\beta) - E\cos(\theta+\delta)$$

$$N_f = \left\{P_a + C - F_1 - H^1 - G_f\tan\varphi^1 + D\sin\delta - (A+B)\sin\beta - X_1\right\}$$
$$\sec\beta\cos(\theta+\beta) + Y_2\sin\theta - X_2\cos\theta + (A+B)\sin(\theta+\beta) + E\sin(\theta+\delta)$$

$$P_{cf} = \sqrt{T_f^2 + N_f^2}$$

$$T_f^1 = T_f - Bn$$

In which

$$Z = \frac{n^2\cos^2\theta}{\left\{n^2-(\sin\theta-b)^2\right\}^{\frac{3}{2}}} - \frac{\sin\theta(\sin\theta-b)}{\sqrt{n^2-(\sin\theta-b)^2}}$$

$$A = \frac{P_{wf}\,r_w\sin\varphi}{nR}$$

$$B = \frac{P_{cf}\,r_c\sin\varphi}{nR}$$

$$C = W_1\sin\delta$$

$$D = \frac{n-l+c\tan(\beta+\delta)}{n}W_2$$

$$E = \frac{l-c\tan(\beta+\delta)}{n}W_2$$

and

M, W_1, and F_1 are the mass, weight and accelerating force respectively for the piston, rod, and cross head.

m W_2, and X_1, X_2, Y_1, Y_2 are the mass, weight and accelerating forces for connecting rod (1) for wrist pin and (2) for crank pin; also n R its length; c R, the perpendicular from its centre of gravity on its centre line; l R, distance of this perpendicular from the wrist pin; K R principal radius of gyration.

R is the length of the crank and τ its angular velocity, and b R a perpendicular from the centre of its shaft upon the path of the wrist pin; δ being the angle of this path with the horizontal.

r_w, r_c are the radii of the wrist and crank pins and tan φ the coëfficient of friction, tan φ' being that of the cross head.

P_a is the total effective pressure of steam; H' friction of the piston and rod; G_f normal guide reaction; P_{wf} and P_{cf} pressures of the wrist and crank pins against the rod; N_f and T_f components of P_{cf} parallel and perpendicular to the crank; T_f' the latter reduced to the centre of the crank pin.

THE PANAMA CANAL AS IT IS. By Dr. WOLFRED NELSON, 348 Broadway, New York.

[This paper will be published in the author's "Five Years in Panama."]

SECTION E.

GEOLOGY AND GEOGRAPHY.

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ADDRESS

BY

GEORGE H. COOK,

VICE PRESIDENT, SECTION E.

ON THE INTERNATIONAL GEOLOGICAL CONGRESS, AND OUR PART IN IT AS AMERICAN GEOLOGISTS.

WE meet here to-day as members of the American Association for the Advancement of Science. The wide extent of this field has made it necessary to consider science in separate divisions. Geography and geology have been assigned to us and we are required to do what we can to advance these two branches of the great subject.

What can each one of us do to help on the knowledge of these, our chosen subjects of study? It is hardly in place here to go over the advances which have been made in geography and geology during the last year. They have been many and important, but the public prints are so widely distributed that it would be only a twice-told tale to go over the subjects in detail. There are, however, some particulars to which it may be profitable to call your attention. This association, at the meeting in Buffalo in 1876, "appointed a committee" to consider the propriety of holding an International congress of geologists at Paris during the International exhibition of 1878, for the purpose of getting together comparative collections, maps and sections, and for the settling of obscure points, relating to geological classification and nomenclature.

Through the efforts and influence of this committee a congress was held in Paris in 1878, at which representatives from this country and from almost all the countries of Europe were present and took part in the proceedings, and the business of the congress as

indicated above was fairly begun. A second meeting was held at Bologna, Italy, in 1881, at which some progress was made. A third at Berlin in 1885 and the points of business for the congress were somewhat further defined. A fourth meeting is to be held in London in September of this year and it is to be presumed that still further progress will be made in the two important subjects before it, classification and nomenclature.

But a meeting of the congress must be held in this country and American geology must be fully represented before any conclusions can be reached which will be accepted by the scientific world. At the meeting in London, an effort will be made to have the next meeting, that of 1891, held in this country. There is good reason to ask that a meeting be held here before the discussions on the important topics under consideration are closed. We think our field of observation an important one, better than that of any of the countries of Europe, and perhaps better than all combined. This was the opinion of the older geologists. William Maclure in his "Observations on the Geology of the United States of America," read before the American Philosophical Society in 1809 gives several reasons why geology can be best studied in America. Prof. Amos Eaton in 1828 quotes De Luc as saying that "the general strata must be settled in America where nature seems to have executed her works upon an enlarged scale." Such, too, is the opinion of some of our active geologists of the present day, whom if allowed, I might quote. But no argument on this subject is needed before this audience, nor will any be needed, to bring the International Geological Congress to the same conclusion, and we may look for it here three years from this time.

With this early notice of what is expected of us, it becomes us to make our preparations to show what we have done in geography and geology and to enforce their claims to acceptance, as part of the material to be used in providing for uniform classification and names. As a profitable way of beginning our work, we inquire What are the points in each of these sciences which are settled and what still remain to be worked out?

And first of GEOGRAPHY. This science, which has been the subject of study for all of us in our school-boy days, still needs great improvements. As a matter of civil geography, our country has suffered greatly in finances and we have come to the very verge of war, from an inaccurate knowledge and description of our bounda-

ries. The northeastern boundary between the United States and Great Britain, described in the treaty of 1783, was remarkable for its inaccuracies of reference to rivers and highlands. It took twenty years to find out the river meant for the starting point and forty years more to find the highlands mentioned in the treaty, and the matter was finally settled by compromise in the Webster and Ashburton treaty of 1842. The boundary line between the United States and Mexico, as described in the treaty of Guadalupe Hidalgo in 1848, was equally inaccurate in its reference to places as they were laid down on an existing map which was appended to the treaty. There were errors of two or three degrees in the projection of the map, so that it did not conform to the topography of the country, and the text of the treaty could not be executed, and ten million dollars were paid to the Mexican government for the strip of territory needed to correct the inaccuracy of description.

The territory comprising the present states of New York and New Jersey was granted by Charles II to his brother James, Duke of York, in 1664, and the same year the Duke sold the portion named New Jersey to two English gentlemen. The partition line by which it was cut off, was probably described from a Dutch map of that time and gave the position and latitude of the two ends of the line which was to be a straight one. It was soon found that the latitudes were in error to the amount of almost twenty miles. This was the cause of litigation and strife for more than a hundred years. The line was finally established by a commission made up of the ablest surveyors from all the then colonies. The line was run and marked by monuments at the end of every mile. Now, however, it is found that no three of these are in a straight line, and taken as a whole, the middle of the line is nearly half a mile out from the line as defined.

Errors of the same kind are to be found in all the states surveyed before the present system of government land surveys was adopted; and they are still the cause of innumerable quarrels and lawsuits. It is true these are not the points of interest in our association, but they furnish most potent reasons for making accurate maps and they cause the supplies to be granted for making such maps,—maps with latitude and longitude accurately determined, and with topographic features correctly located, such as are essential to good work in geology. The forms of hills and mountains give clews to their origin and structure—the occurrence of

gaps or depressions in ridges suggests searches for faults—breaks in the lines of ridges lead to examinations for offsets,—lines of rivers and of drainage lead to inquiries in almost every line of investigation. With a good topographic map before him a geologist can lay out his plans for attacking the different problems which are to be solved, or he can start his plan of the hypothetical structure of a region, and select the points he needs to study, in order to confirm or confute his hypothesis. Such maps are indispensable in laying down and describing the localities of the outcropping rocks which make up the geological series of the whole country. The work of making such maps, much as they are needed and imperatively as they are called for, is only just begun. The United States Coast and Geodetic Survey has long been engaged in determining the geographical positions of prominent points in the country bordering on the shores of the Atlantic, the Pacific and the Gulf of Mexico, and, as far as sustained, it has carried the work inland, and if properly supported it will carry its stations across the continent. Its results are attained with the greatest degree of accuracy and are recognized as authority in all countries where such surveys are made. As a basis for all reliable geographical surveys and descriptions, this work of the Geodetic Survey is needed now in all parts of our country and its prosecution should be urged as rapidly as possible.

Good work in the same line has also been done in the lake surveys by the United States government. Earlier surveys of the same kind were begun by the state of Massachusetts, and more recently New York has begun a state geodetic survey.

The accuracy with which surveys can be made is only beginning to be appreciated. One has only to go into the courts and learn of the disputes and litigation which have attended the older locations of boundaries and grants of land when errors of miles were frequent, while by modern methods positions can be fixed within a few inches, to understand the need there is for good work everywhere. And it is within the province of this association to exert all its influence to this end. In topography the making of surveys is only just begun. Topographic maps of New Hampshire were prepared under the direction of Prof. C. H. Hitchcock in 1874. Maps of Colorado and other western territories prepared under Dr. F. V. Hayden were published in 1877. The topographic map of New Jersey begun under the direction of the state geologist in 1877

and continued since July, 1884, by the aid of the United States Geological Survey, is just completed and published. The topographic survey of Massachusetts begun by the joint action of the state and of the U. S. Geological Survey in 1884 is now approaching completion.

The United States Geological Survey began systematic work looking to a topographic map of the whole United States, several years since, and it is now in progress in different sections of the Union. The maps are being engraved in the best manner and issued as fast as they are completed.

There are other topographic maps of more limited districts, to which reference might be made, but those mentioned are sufficient to indicate the progress made in this direction. We are far behind the countries of Europe in respect to maps of the whole country—but it is believed that our later maps will not suffer in comparison with the best of those of foreign lands, and from some experience in directing such surveys, I feel warranted in saying that no public expense, incurred in carrying on scientific explorations, meets with such hearty recognition and approval as that for making and publishing such information in regard to the topographic features of the country in which we reside or travel. Such maps are invaluable to the civil engineer, to those watching over the public health, to those projecting works for water supply or for drainage, to those looking to the location of roads and railroads, to those seeking locations for rural homes, to those studying routes of travel on foot, by carriage or by bicycle, and they touch at some point or other the interest of every thoughtful or inquisitive citizen. Public money faithfully and judiciously expended in this direction gives encouragement to those controlling the public purse, to make liberal provision for the continuance of scientific works, and to aid in the investigation of the facts and principles which constitute the bases of all true science.

To us, however, geography is of most interest, because the forms and features of the earth's surface are so connected with the structure and materials which underlie it, that they furnish a guide to direct us in our geological studies, and a means of recording their results with accuracy and clearness. While there is much to be done, there is great encouragement to go on when the subject is progressing so satisfactorily.

GEOLOGY is revealing to us so many of its hitherto hidden treas-

ures, that its study is being pursued with more ardor and intelligence than ever before.

Geology which treats of the structure of the whole earth, and which includes in its domain facts ascertained and principles deduced from all its parts, was first systematized from a very limited portion of the globe. It is not surprising, that a system arranged consistently with the facts in a single country should not be comprehensive enough to meet the circumstances of all others. American geologists began by transferring the German, English and French systems to this country. It took little time to find they did not fit the circumstances here, but with that reverence for authority which is due from the younger to the older, we have been trying to make our geology conform to theirs. The effort is only partially successful; and we have to admit, that something larger and more far-reaching must be devised before the science can be called a general one, applicable in all places.

It was probably some clear perception of this want in the science which led our fellow members to move for an international congress of geologists, and now it is our part to see where the deficiency lies, and to do what we can to make preparations for supplying it.

The time is so short since geology was first studied in any systematic way in this country, and the advances have been so rapid and so large that some consideration is needed before we can clearly define our present position and standing. It is within the lifetime of some of our still active geologists, that our first books on the subject were written. Maclure's "Observations on the Geology of the United States of America," begun in 1809, was first printed in its full form in 1817, and contains only about thirty pages of descriptive matter on American geology. It was, as Professor Silliman said, "a grand outline, drawn by Mr. Maclure with a masterly hand and with a vast extent of personal observation and labor." In 1818, Prof. Benjamin Silliman established the American Journal of Science and Arts, and in his introductory remarks he says "To account for the formation and changes of our globe by excursions of the imagination, often splendid and imposing, but usually visionary and almost always baseless, was, till within half a century, the business of geological speculations, but this research has now assumed a more sober character; the science of geology has been reared upon numerous and accurate observations of *facts;* and

standing thus upon the basis of induction it is entitled to a rank among those sciences which Lord Bacon's Philosophy has contributed to create.

"The outlines of American geology appear to be particularly grand, simple and instructive, and a knowledge of the important facts and general principles of this science is of vast practical use as regards the interests of agriculture and the research for useful minerals. Geological and mineralogical descriptions, and maps of particular states and districts, are very much needed in the United States; and to excite a spirit to furnish them will form one leading object of the contemplated journal."

From this date onward the growth of American geology has been rapid and plainly marked. The American Journal itself, still in the charge of the family of its founder and supported by a corps of men representative in science, continues to be a repository of the advances of geological science. The Academy of Natural Sciences began the publication of geological papers the same year, and Prof. Amos Eaton published his first geological paper that year. It was a geological transverse section extending from Catskill mountain eastward to the Atlantic. Prof. Edward Hitchcock began the publication of his geological studies very soon after. The "Geological and Agricultural Survey of the district adjoining the Erie canal in the state of New York, accompanied by a Geological Profile extending from Boston to Lake Erie," by Prof. Amos Eaton, was published in 1824. The Geological Survey of North Carolina was begun by Denison Olmstead in the same year; that of South Carolina by Lardner Vanuxem in 1826; that of Massachusetts by Edward Hitchcock in 1832; that of Maryland in 1834 by J. T. Ducatel and J. H. Alexander. That of Tennessee by G. Troost in 1835. In 1836 surveys were begun in New York by Wm. W. Mather, Ebenezer Emmons, Lardner Vanuxem, Timothy A. Conrad and James Hall; in New Jersey by Henry D. Rogers; in Pennsylvania by Henry D. Rogers; in Ohio by S. P. Hildreth, and in Virginia by Wm. B. Rogers. In 1837 Charles T. Jackson began the survey of Maine and Jas. G. Percival that of Connecticut. In 1838 D. D. Owen began that of Indiana, and Douglass Houghton that of Michigan. Delaware was begun by Jas. C. Booth in 1839 and Kentucky by W. W. Mather in the same year. Rhode Island was begun in 1840 and New Hampshire in 1841 by Dr. Charles T. Jackson.

The bare enumeration of these surveys and reports is enough to show the active interest in geology which had been developed in the country in those twenty-three years, both in the estimation of scientific men and in that of the public which authorized and encouraged the surveys. The men prominent in prosecuting the various surveys were able, enterprising and enthusiastic in their views of geology, both in its relations to science and to its economic applications. Their reports, now at the end of fifty years, are interesting reading and we wonder that so much good work could be done in a new and broad field, with such imperfect information to guide and with moderate financial support. The publication of these reports made it evident that men engaged in fields so remote from each other would be benefited in their work and relieved in their perplexities by conference with each other. On April 2, 1840, a meeting was held in Philadelphia and the American Association of Geologists and Naturalists was organized. Of the eighteen present, thirteen or fourteen were geologists fresh from the field. Nearly all have finished their work, though we are still permitted to see one of them,—Prof. James Hall, ripe in years, vigorous in health, and alive to everything which promotes the interests of the subject to which he has devoted himself for more than half a century.

The proceedings of the meeting, which was continued through a second and third day, are of interest to us, as showing the problems which occupied them, something of the questions then settled, and of those on which they sought information and advice. Professor Hitchcock exhibited specimens of "fossil footmarks so called." "This subject was of such interest as to induce the Association to appoint a committee to visit the localities and to report at the next meeting." Some persons had insisted that the *tracks* were only markings of some nondescript seaweed; but next year the committee, H. D. Rogers, Vanuxem, Taylor, Emmons and Conrad, reported that the views of Hitchcock were sustained by facts, and that the tracks were those of birds as he represented. Later discoveries have proved the then existence of Dinosaur reptiles, which were "biped in locomotion, like birds, seldom bringing their fore-feet to the ground." Some of them were three-toed, others four-toed, and still others five-toed. And the question involving so much of interest, and to some of feeling, is now settled in the minds of all geologists, the tracks are those of reptiles.

At this first meeting there was a discussion on the subject of diluvial action. Information was communicated concerning diluvial grooves or scratches observed in the valleys of the Hudson, the Ohio and the Mississippi, on the polished limestones in New York, Pennsylvania, etc., and points were suggested for investigation. At the following meeting, W. W. Mather, who appears to have been of a committee on Drift, asked leave to defer his report to the next meeting. The subject, however, was ably discussed then, and the report of the discussion is very interesting showing, as it does, the interest it excited, and the many inquiries and investigations it had set in operation.

At the third meeting, the subject was a leading one before the Association. A very full and admirable paper was presented by Mr. Couthoy on the phenomena of icebergs, their movements, their agency in carrying earth and stones, and in disturbing the bottoms of shallow seas. A very full and carefully prepared paper was read by Professor Hitchcock on "The Phenomena of drift, or glacio-aqueous action in North America." His statement of facts was full and very fair, and approached as near to the glacial theory as could be done by one who had not visited glaciers and observed their phenomena. Charpentiér's work on glaciers, and the erratic deposits in the valley of the Rhone, was then but just published, and the masterly demonstrations of Agassiz on glaciers, their movement and effects were not completed when this meeting was held. It is not surprising that our geologists were slow in accepting this theory. There were no known glaciers in the country, and we had nothing in our observation from which to get a starting point. Gradually as more students have visited European glaciers, and their phenomena have been more studied, the theory has been accepted, and those who are still incredulous are a small minority. It has been the subject of many papers and much discussion in this Association; and, as would naturally be the case, the question has been most clearly presented and most convincingly settled in those parts of the country where the glaciated and the unglaciated surfaces come in contact. And if the doubters will visit the southern margin of the great terminal moraine and examine the surface where within a few feet one passes from the glaciated surface and the moraine of bowlders, gravel and drift-earth to the unglaciated surface and the plain where bowlders and mixed gravel are almost unknown, I cannot but think they will be convinced. The second

question which has originated in later times, whether there was an earlier glacial formation than the one so well marked and which extended farther south, and that there was a long interglacial period between them, is answered in the affirmative by many. It is a question surrounded by many more difficulties than the first and I think needs more complete demonstration. It is worth the attention of our geologists and we may hope it will be settled before 1891.

Another question presented at the first meeting of geologists and naturalists was as to the origin of serpentine, whether strictly intrusive or metamorphic. The question has been much discussed since that time and some of its difficulties have been cleared up. Serpentine is now known to be a secondary product by chemical change and substitution of some of its elements, both in igneous and stratified rocks. It is in some cases not easy to assign it to its proper origin and each exposure of the rock in place has to be determined by its own characters and surroundings.

Other subjects of a more general character relating to agriculture and the arts were presented and discussed. It is a source of regret that a fuller report of the proceedings was not prepared and published; for this meeting marks an important era in the history of geological science in America. The conference gave new strength and courage to investigators; it introduced new men into the field; it began the work of combining and systematizing facts which were ascertained; and of providing for the newly discovered facts as they were brought forward. We owe a debt of gratitude to the men who took such pains to learn to do their work well and to transmit it in well arranged form to their successors.

Meetings were held by the Association in the successive years 1841 to 1847 inclusive, and it was then resolved into the American Association for the Advancement of Science, the first meeting of which was in 1848. The section of Geography and Geology, now section E of the American Association, is the representative of the society organized by American geologists to collate the individual work of each other, and to bring them into harmony of succession and name. It has already done much, and has reached the position from which it is prepared to do much more.

Many and perplexing questions have arisen in the progress of geology, some of which have taxed the powers of our ablest men; by continued efforts they are being solved. The Taconic ques-

tion was really started as early as 1828 when Professor Eaton, in an article on Geological Nomenclature, in the Am. Jour. of Science, Vol. 24, p. 147, said "I have traversed the transition range from Massachusetts line to Hudson in fifteen places since the first part of this survey was published, for the purpose of ascertaining the true superposition of rocks in this most complicated and difficult geological theatre.

"The argillite under which the granular limestone passes near Massachusetts line, is certainly the very same continuous rock which forms the Cohoes falls and bed and banks of the Hudson from Baker's falls to Newberg near the Highlands. All the intervening rocks lie in a kind of inclined trough in the argillite. *We have no primitive argillite in our district, if organic remains form the characteristic distinction.* Neither do I believe there is such a rock as primitive argillite on this globe. This is Bakewell's opinion; and though I have often changed mine, I now believe he is correct, and that the basseting edges of the same rocks present a more primitive appearance in all cases; and that this fact has led geologists into ruinous errors."

These were the opinions reached by Professor Eaton; they vary somewhat from those presented by him in his report on the Erie canal rocks in 1824; but they were his matured conclusions and from them he never afterwards varied. He presented them strongly as became one who was thoroughly convinced of their correctness. At the end of sixty years we can now see for the ground he worked on, that he was right; and if he had known of some peculiarities of structure which have been worked out by his successors he could, with proper allowance for faults, have made a demonstration which would have saved a vast amount of time and labor. In 1842, Dr. Emmons presented his views of the rock structure of the country between western Massachusetts and the Hudson river and gave to the rocks the name of the Taconic system. This was in accordance with the teachings of Professor Dewey in 1820, Am. Jour., Vol. II, p. 246. This view was controverted by Dr. Emmons' associates in the New York Geological Survey and by many other prominent geologists; on the other hand it had the support of some, and the discussions on the subject have been earnest and well contested almost up to the present time. It is now conceded that the array of evidence brought by Prof. J. D. Dana, and later by Mr. Walcott, proves the case against Dr. Emmons and his Ta-

conic system as first stated. The slates and limestones along the New York and New England boundary are of the Silurian age. Dr. Emmons, in his later work, did describe some rocks of the pre-Potsdam age, but they are on the western border or almost outside his original Taconic system of rocks. It now remains to assign the proper value to the name Taconic or to abandon it. Its originator was an able man and an industrious investigator. The questions he introduced were fair ones and have been well contested; if they have not been fully proved in the affirmative they have at least brought him no dishonor. In the progress of this discussion it appears that some of the difficulties attending the case arose from the different degrees of metamorphism in rocks of the same age and composition. Attention is now drawn more strongly than at any previous time to the need of a careful study of the metamorphic changes which rocks have undergone.

Prof. Henry D. Rogers, at one of the early meetings of the Association, brought forward the peculiar structure of the Triassic formation of New Jersey, Pennsylvania and the states farther south; a long and comparatively narrow strip with a monoclinal dip nearly at right angles to the length of the formation. The origin of this was to him a subject of curious interest, and his presentation of a theory to account for it, was a delight to those who could listen to his eloquent delivery. His theory was not satisfactory, however, and many have since tried their skill in the endeavor to solve the problem. It has been supposed that if longitudinal or northeast and southwest fractures, with an upthrow on the northwest side of each of them, could be found that they would explain the difficulty; but the rock is so nearly uniform in color and composition and so easy of disintegration that faults are hard to find, if they do exist. But it is believed that localities have been found where such faults can be shown and the fractured strata identified, both by mineral composition and by fossils. This removes another vexed question from the field and leaves the geologist to turn his attention to others that demand investigation.

Where to place the American Trias in the geological column is not settled. There are also further questions relating to the age of its igneous rocks, on which there is some difference of opinion; and the correlation of the Trias of the Atlantic slope with that of the middle and western portions of the continent needs much examination and study to bring it to a satisfactory condition. The

materials collected by the geologists of the United States Geological Survey and by those of other government surveys have made large provision for such examination and now await the final arrangement.

The labors of Dr. Morton and others, early in 1834, demonstrated the existence of the cretaceous system in New Jersey and states farther south, and the study of its fossils in New Jersey, by them, has made that locality classic ground for studies in that member of the geological column. The developments there, though plainly and carefully worked out, comprise but a small part of the system as found in other countries. It is interesting to see the results of the explorations of later times as they have been continued at the south and in the great plains west of the Mississippi, in the Rocky mountains and in California. Each of these fields supplies some missing member of the series. The additions made are enormously large and seem to give all that is needed to make the system complete. The rocky structure of the country needs now to be studied to make sure that there are no repetitions of the rocks by great faults which sometimes increase the apparent thickness to a wonderful degree. Dr. White, in the last number of the American Journal, published his conclusion that the Laramie belongs in the upper part of the Cretaceous. At our meeting last year, Mr. Hill gave an account of the great additions to this system in the geology of Texas, and we have from Mr. McGee an account of a new addition to the lower portion of the Cretaceous in the vicinity of Washington which he names the Potomac series. With the scattered material which has been published in various reports and otherwise, there should be enough to show the fulness of the American series of that system.

In the Tertiary a good beginning was made early in the explorations of our Atlantic and Gulf borders, but those in the west and in the Rocky mountains are so much more extensive than any before known, and the fossils described are of such an extraordinary character, that it seems that a revision of the series, heretofore known, must be made so as to include the American developments.

Whether the Quaternary shall appear as a system in the list, or shall simply be considered as one of the Tertiary series is an unsettled point. If the systems are to be distinguished from each other by well and plainly marked changes in stratification or

structure, then it is most likely the Quaternary will be only a system in that group.

In the International Geological Congress the two topics for examination and if possible for agreement are the general system of nomenclature, and the colors to be used in making geological maps. It is, however, perfectly obvious that before agreeing on names to be used, the objects to be named must first be agreed upon. And it is evidently from the lack of completeness in the geological column in any single country where the geology has been well studied and described, that the first difficulty arises. The order of succession of the rocks has been published, and names have been given to them, and now that these have been in use it is difficult to so change them as to make them a part of a scheme that shall be of universal application. It was this end which our Association aimed at in their resolution passed in 1876. And while progress has been made in the work at each meeting since held by the Congress, it is still in a very mixed condition. Great difficulties arise from the different languages spoken by the representatives of the several nations represented. In the Congress at Paris, in 1878, twenty different ones represented; in that at Bologna, in 1881, there were sixteen, and in that of Berlin in 1885, seventeen countries were represented. The language of the Congress is French, and all the representatives are required to present their subjects and communications in that language. Of course all foreigners are at a disadvantage,—and many are hopelessly so. Another difficulty arises from unequal representation. The attendance is voluntary, the members pay their own expenses, and the time and money required must hinder many who are deeply interested from attending the meetings, and this hindrance is greater in proportion as the distance from the place of meeting increases. The attendance shows this; at the Paris meeting there were 194 Frenchmen and 110 foreigners, at the Bologna meeting 149 Italians and 75 foreigners, and at the Berlin meeting 163 Germans and 92 foreigners. This it will be seen does not give general geology a fair representation, when questions come up which are to be decided in favor of the majority voting on them. Such votes can only be tentative, and the decisions will hardly be acquiesced in until a more equable representation is brought to act upon the unsettled questions, and many more countries have been fully represented. They do, however, bring out the questions upon which action is to

be taken, and prepare the way for a right decision. The Congress at Berlin aimed to embody the present condition of European geological science, and cartography by preparing a map of Europe in which the legend gave all the larger known divisions of the geological column, and the colors on the map showed their locations. The following tabular statement gives the results they had reached at the time of the Berlin meeting in 1885. It is taken from the Report of Dr. Persifor Frazer, Secretary of the American Committee of the International Geological Congress, who was present and took part in the meeting. I copy only the classification and nomenclature, and omit the part relating to color.

The presentation of this tabular statement gives opportunity to examine the various parts separately; allows those who make particular portions of it their study, to fill up its deficiencies and those who are in any way questioning its truth to nature, to make criticisms and corrections. It will be seen that the stratified Terranes are all included in five divisions which go from the more general to those which are more specific.

Those of the

First	order	are	named	Groups,	and	their	times	are	Eras.
Second	"	"	"	Systems	"	"	"	"	Periods.
Third	"	"	"	Series	"	"	"	"	Epochs.
Fourth	"	"	"	Stages	"	"	"	"	Ages.
Fifth	"	"	"	Beds	"	"	"	"	?
Sixth	"	"	"	?	"	"	"	"	?

SEDIMENTARY TERRANES.

TIME DIVISIONS.				
1st order. ERA.	*2nd order.* PERIOD.	*3rd order.* EPOCH.	*4th order.* AGE.	*5th order.* ?
STRUCTURAL DIVISIONS.				
GROUPS.	SYSTEMS.	SERIES.	STAGES	BEDS.
Cenozoic.		Present deposits (alluvial, etc.)		
	Tertiary.	Quaternary (diluvium)		
		Pliocene,		
		Miocene,		
		Oligocene,		
		Eocene.		
Mesozoic.	Cretacic.	Upper Cretacic,		
		Gault,		
		Lower Cretacic (Neocomian),		
		Wealdian.		
	Jurassic.	Malm,		
		Dogger,		
		Lias.		
	?	Rhœtic.		
	Triassic.	Upper Trias (Keuper)		
		Middle Trias (Muschelkalk)		
		Lower Trias (variegated sandstone).		
?	?	Zechstein,		
		Permian.		
Paleozoic.	Carbonic.	Coal Measures,		
		Lower Carbonic (Culm),		
	Devonic.	Famennian,		
		Eifelian,		
		Rhenan.		
	Siluric.	Upper Siluric,		
		Lower Siluric,		
		Cambrian.		
Azoic.	Archæan.	Azoic Schists (Phyllites),		
		Crystalline Schists,		
		Gneiss, etc.		

There was strongly expressed opposition to the order of the terms *groups* and *series*, a very respectable minority preferring to have them transposed and to call divisions of the first order *series* and those of the third order *groups*, and this action may yet be taken. The names of the several groups are well settled by present usage, though the French and some other nations preferred the still older names of Primitive, Primary, Secondary and Tertiary; and as they are well settled in our language they might be reinstated without difficulty.

The divisions of the second order, *systems*, are the same with those now in general use. The attempt is made to have the names all end in *ic* and is successful except in the first and last ones. It will be seen that no agreement was reached in regard to the proper location of the dividing line between the Jurassic and Triassic systems, the Rhœtic series being the subject of difference. It is doubtful whether it is distinctly recognized in American geology. And in the cases of the Zechstein and Permian, there was such decided difference of opinion as to their proper connection with the Triassic or Carbonic systems that the decision was deferred. Our American geologists, Hall and Newberry, who were present, stated that, as far as now known, the decision would not affect the division of the American series, no representatives of the Permian having been found in the United States. The clearest sentiment, as expressed, was rather in favor of joining them to the Carbonic system.

As far as divisions of the first and second order are concerned, it appears as if uniformity of practice in marking the divisions, in giving names and in using colors for maps are concerned, a conclusion could soon be reached. But in the divisions and names of the third order, as they now stand in the Proceedings of Congress, there will need to be much further investigation and conference. But the free and wide fields of research and discussion must certainly bring out the truths now wanting in order to fill out the blank places in the series. The extension of work over wider fields is constantly filling the great gaps which are so marked in narrower fields, and are leading to the conclusion that while sedimentation has ceased for a time in some localities, it has at the same time been going on in others; and so the passage from one division to another may finally be shown to be an almost inappreciable one.

As it stands now in this list of names we are reminded of the remarks of Whewell made more than fifty years ago, that the advancement of three of the main divisions of geological inquiry has during the last half century been promoted successively by three different nations of Europe, the Germans, the English, and the French. The systematic study of what may be called mineralogical geology had its origin and chief point of activity in Germany, where Werner first described with precision the mineral characters of rocks. The classification of the secondary formations, each marked by their peculiar fossils, belongs in a great measure to England, where the labors of Smith, and those of the most active members of the Geological Society of London, were steadily directed to these objects. The foundation of the third branch, that relating to the Tertiary formations, was laid in France by the splendid work of Cuvier and Brongniart, published in 1808, "On the Mineral Geography, and Organic Remains of the Neighborhood of Paris."

"We may still trace, in the language of the science, and our present methods of arrangement, the various countries where the growth of these several departments of geology was at different times promoted."

With the great accessions which have been made to the general stock of geological knowledge, by American geologists, and the general publication of it, it becomes necessary that this should be incorporated in a work which is designed to be comprehensive enough to take in the geology of the world. This list of names for the members of the series undoubtedly satisfied the Europeans who voted upon them, but they are too local, too geographic, too strange to have a place in any general series. Names must be given in describing new kinds or occurrences of rocks, but they should be provisional, and dropped whenever some more characteristic or generally appropriate name can be found. For calling attention to the several divisions, these names will be very useful, and by their general publication they can be brought to the consideration of hundreds of working geologists, who by their contributions and suggestions can throw light on the subject, though they may never be able to attend an international geological congress. The advancement of science in modern time is brought about much more by the increased number of workers in the cause than it is by the greater attainments of a few men. With attention properly

drawn to this position of geological science, with a great body of workers in the field, with an immense territory in which to work,— and with a notice of three years in advance, we can prepare the case so as fairly to present the claims of American geology to a representation in a general system of geology. The Congress went no farther in the lists of names: those of the fourth, fifth and sixth order will be still more difficult to generalize, and it may be that it will be found expedient to leave the names of these orders to be given in the languages of the countries where they find their application.

It might tend to a more equitable representation of the views of members from different countries, if the number of votes to which each country should be entitled, could be equitably settled, and the representation from each country should be in some way controlled by the whole body of geologists; but in a country like ours, where most geologists have active duties to discharge in the milder seasons when meetings are held, this cannot always be had. Besides, the work calls for an individual sacrifice of money and time which many persons think they cannot properly make either for the public good, or for the benefit of science.

These are difficulties which attend the present arrangements for work; and at present I can only bring them to your attention without offering any suggestions for their solution. The objects of the Congress are worthy and useful ones, and they will be attained. To us they give direction and point to our investigations and studies, and they will be profitable by leading us to a fuller examination of the whole field of geological science as well as to a more careful and demonstrative study of special fields in which our individual work lies.

PAPERS READ.

THE DISCOVERY OF SPOROCARPS IN THE OHIO SHALE. By Prof. EDWARD ORTON, Columbus, Ohio.

[ABSTRACT.]

AT the Montreal meeting, I described to the members of section E the occurrence in vast numbers of microscopic spores in the shales of Devonian age in Ohio and adjacent states. I have recently obtained a new line of facts in regard to these interesting bodies, in the discovery of the *sporocarps* or the vessels that contained these spores.

In 1863, Sir Wm. Logan noted the occurrence of certain fossils which he characterized as "microscopic, orbicular" bodies in the Upper Erian shales of Kettle Point, Lake Huron, but he made no further reference to their occurrence for a number of years. In 1869, he called the attention of Principal (Sir J. William) Dawson to these bodies, and the latter afterward described them in his report on the Erian flora of Canada (Geol. Surv. of Can., 1871) under the name of *Sporangites Huronensis.* He counted them the spores of acrogenous plants and presumably of *Lepidodendron primævum*, the remains of which he found in the same beds.

In the April number of the Amer. Journal of Science for the same year, Principal Dawson called further attention to these forms, noting their resemblance to certain fossils from Brazil described by Carruthers as *Flemingites*, and also to fossils from the white coal of Australia.

These shale fossils had been meanwhile rediscovered and studied elsewhere, viz., as they occurred in the bowlder clay underlying Lake Michigan where the tunnel for the water supply of Chicago was driven. Dr. H. A. Johnson and B. W. Thomas, Esq., studied them, figured them, and sent them to distinguished microscopists of this country and Europe without however obtaining a clew to their real nature. The occurrence of the spores in the city water of Chicago was also noted by Mr. Thomas.

In 1879, without knowledge of the previous discoveries, I found these fossils once more, in the drillings of a deep well, 800 to 1000 feet below the surface, in Kingsville, Ohio. I made no advance in my knowledge, however, for two years, but at the end of this time, I came upon them again

and learned for the first time their universal distribution through the entire shale formation of Ohio, Kentucky, Indiana and Michigan. By correspondence with Sir Wm. Dawson, I learned of his priority in the discovery and interpretation of these fossils. I adopted his name of course, but the facts presented in my paper at the Montreal meeting led him, as he says, to revise his conclusion as to the origin of the spores and to suspect their derivation from some group of aquatic plants lower than the Lycopods.

Professors H. S. Williams and J. M. Clarke contributed some facts as to the occurrence of these bodies in the shales of New York.

The next step in advance was taken in 1883 by Sir Wm. Dawson. He received at that time from Brazil, through Mr. O. A. Derby, specimens which threw new light on the whole investigation. Sir William had previously received *Sporangites* from that country in collections made by Professor Hart, but Mr. Derby's specimens afforded not only the spores, but the envelopes in which they were contained, which occurred as oval sacs. These sacs were found by Sir William to resemble very closely the sporocarps of *Salvinia*, a member of a somewhat insignificant group of acrogens, known as rhizocarps. The name *Protosalvinia* was accordingly brought in to displace the earlier designation, viz., *Sporangites* and we now have five forms referred to the new genus. The original form, *P. Huronensis*, occurs in vast numbers throughout the entire series of the Ohio shales.

In April, 1888, the next advance was made in our knowledge of these forms by the discovery made by one of my students, C. J. Welch, E.M., of sporocarps in the shale series of Columbus, Ohio. The first found were flattened, circular discs, about 4 mm. in long diameter, composed of thick walled, carbonized cells, beautifully reticulated in structure; but, presently, a new and distinct form, elongated and furcated, was found by Mr. Welch, which has been named by Sir Wm. Dawson, *Sporocarpon furcatum*.

Both forms are found in great abundance in certain phases of the shale formation and new light is promised by them as to the rhizocarpean vegetation from which they are derived.

Newberry's suggestion that we owe the black shales to a Sargasso sea was a fruitful one, matching the facts better than any other theory of origin. At last, we see the kinds of vegetation that mantled these land-locked basins of the early days. It did not consist of algæ, but belonged to a higher division of the vegetable kingdom, viz., the rhizocarps. It is thus seen to be closely allied to the divisions of plants that were covering the land at this time with a wonderful growth of ferns, Lycopods and Calamites.

The vegetable kingdom is divided into two main groups, viz., Phænogams and Cryptogams. The first is by far the more important at the present time. To which do we owe the great accumulations of the stored sun-power of the past as found in coal? To the latter division. But the Cryptogams are broken up into three main series, viz., Acrogens, Anogens and Thallogens. To which of these is the work of coal accumulation due?

Mainly, to the Acrogens. The latter are subdivided into the following families: *Lycopods*, *Ferns*, *Scouring-rushes* and *Rhizocarps*. From which of these is the coal flora derived? Mainly, from the first three.

There is left a single division that lacks prominence at the present time, but which we are now following back to a widespread development and to a most important service. The highest office of the vegetable kingdom consists in its storing up the power of the sun. The two permanent forms of stored sun-power are coal and petroleum. Both of them, we owe to acrogens, coal to the terrestrial, and petroleum to the marine representatives of the class. The shale series which we have been considering is as unmistakably the great source of the accumulations of oil and gas in Pennsylvania and New York as the carboniferous formation is the main source of coal.

THE NEW HORIZONS OF OIL AND GAS IN THE MISSISSIPPI VALLEY. By Prof. EDWARD ORTON, Columbus, Ohio.

[ABSTRACT.]

IN the epidemic of drilling deep wells that has swept through the states of the Ohio valley during the last five years, a vast store of facts of great geological importance has been brought to light and some of them are also immensely important on the economic side. In all this work it is the unexpected which has happened.

Four or five horizons from which oil and gas are derived on a large scale have been added to those already known.

1. The first in value is the Trenton limestone. This is at the present time the most prolific single source of bituminous products in the entire country. Wells reaching down to it yield a maximum of thirty million cubic feet of gas per day and maintain their flow for months and years without unusual reduction. Wells also are found in it that produce 5000 barrels of oil in a day and that keep up their flow until totals of more than 100,000 barrels are credited to single wells. The largest connected gas territory in the world derives its supplies from this horizon. The structure of the new fields is so simple and easily read that important light is being thrown upon the problem of gas and oil accumulation everywhere from the facts that obtain here.

The porosity of the rocks, upon which its storage capacity depends, is due to a dolomitization of 5 to 50 feet of the uppermost beds of the stratum. To the imperfect interlocking of the dolomitic crystals, the storage chambers of the gas and oil are due. The areas in which this replacement has taken place can be pointed out. Most of the territory is, of

course, occupied by salt water, but in the domes or terraces where relief is furnished, the bituminous products are gathered.

In production, every foot of relief in the storage rock proves effective. The spirit level gives, in most instances, a good explanation of the behavior of different wells.

The depth of the reservoir is found to be a function of the rock-pressure of the gas or oil. All the facts seem to show that this rock-pressure has an artesian origin, taking its rise in the salt-water that lies behind and below it. The latter element is thus seen to be an essential one in every region of high-pressure gas.

As to the present production of the Trenton limestone it is only necessary to say that many hundred million cubic feet of gas are surging forth every day throughout no less than 3000 square miles of territory in Ohio and Indiana. Upon this enormous production of light and heat and power, a vast manufacturing interest is being rapidly built up, which is certain to work revolutionary changes in many interests. The supplies have been wasted in a most extravagant way, but they will last long enough to shift the centres of manufactures to quite an extent.

The gas is piped with great success to cities forty miles distant from the fields while the oil is carried as far as Chicago in pipe lines, to become the basis of a very important fuel-supply.

The production of oil is repressed as far as possible by bringing down the price to fifteen cents per barrel. In spite of this effort, 20,000 or more barrels are brought to the surface every day, and a price of thirty cents per barrel would bring 100,000 barrels to the surface every twenty-four hours, within ninety days.

2. The well-known stratum, the Clinton limestone, proves to be a gas and oil rock of considerable value in Ohio under suitable conditions of relief. The most important gas supply of the Clinton is found at Lancaster. It is probably to this horizon that the Glasgow production of southern Kentucky is due.

3. The Canada oil horizon is shown by the sections that approach it from Ohio and in Michigan, to be in no way connected with the Corniferous limestone as has heretofore been asserted, but it is buried in the great series of Onondâga-Lower-Helderberg age, presuming these two formations, viz., the Onondaga salt group and the Lower Helderberg limestones, to be of one and the same age, but standing for different conditions of growth.

4. The Ohio shale has recently been found to be a source of high-pressure gas in western Kentucky. If anything could be counted settled as to the gas production of a particular formation, the facts as to the Ohio shale might have been so considered, but most of the experience of the Ohio field in regard to it has been set aside by the facts to which reference is here made. Owing to a porosity that the formation has acquired through some of the accidents of its history, it becomes in Meade county, Kentucky, a true reservoir rock, its gas pressure being determined by a salt-water column as in all other true reservoir fields.

THE CRETACEOUS DEPOSITS OF NORTH AMERICA. By DR. CHARLES A. WHITE of the U. S. Geological Survey.

[ABSTRACT.]

THIS paper comprised a portion of work upon the cretaceous deposits of North America which Dr. White is preparing for publication. When completed it will appear as a Bulletin of the United States Geological Survey and it is therefore only briefly noticed in this volume.

It gave an outline of his proposed work and, by way of illustrating the method of its execution, the paper included that portion of the work which relates to the cretaceous deposits of the Atlantic coast region. The immediate incentive to the preparation of this work is the pressing need of a revised scheme of classification of the geological formations of this continent which shall receive at least the conventional approval of all leading geologists. It is, however, intended that the work, although only a concise summary of what is now known of this subject, shall be of more than temporary use.

For convenient treatment of his subject the author divides the continent geographically into regions and provinces, which divisions are in part natural and in part arbitrary. He will attempt to correlate all the sections of North American cretaceous strata that have been published by different geologists which will exhibit the synonymy of the different formations and their equivalents which have thus arisen.

ON THE OCCURRENCE OF THE "FOREST BED" BENEATH INTRA-MORAINIC DRIFT. By FRANK LEVERETT, U. S. Geological Survey, Madison, Wis.

[ABSTRACT.]

THE region under discussion is a portion of northeastern Illinois which is crossed by a series of moraines of the Lake Michigan glacier, older than the moraines described by Prof. T. C. Chamberlin in the Third Annual Report of the U. S. Geological Survey (though recognized and indicated in part by dotted lines) and since studied by the writer under the direction of Professor Chamberlin.

The "Forest Bed" exhibits a variety of phases: peat, muck, soil, wood, etc. It has been found by well-borings, and samples of these borings have been examined by the writer.

1. *Geographic Distribution.* There are three main belts: (1) Near Mendota, Illinois, over an area about twenty-five miles in length and two to four miles in width, beneath the inner slope of a morainic ridge, at a depth of sixty to one hundred and forty feet, the distance from the surface increasing with the elevation. (2) In the "Iroquois artesian well district"

in Iroquois and portions of adjoining counties (Vermillion, Champaign, Ford, McLean and Livingstone), over a known area of at least five hundred square miles. It is most frequently encountered beneath the till plain on the inner slope of the moraines which sweep around the artesian well district, at a depth of sixty to one hundred and ten feet, but is occasionally penetrated beneath moraines in Livingstone, McLean and Ford counties and is found along the outer border and slopes of moraines in Vermillion and Champaign counties. (3) In McHenry and Kane counties in the midst of a series of morainic ridges at depths varying from sixty to one hundred and eighty feet; the depth increasing with elevation.

2. *Stratigraphic Distribution.* It occurs both as an interglacial soil between tills, and below all the tills as if preglacial. Nowhere have more than two beds been penetrated in the same well section and usually but one occurs.

The "Forest Bed" is known to be *in situ*: (1) By a leached subsoil. (2) By underlying sand beds containing molluscan shells. The subsoil has been found in the Mendota area leached to a depth of two to four feet, beneath which there is a calcareous till. The fossiliferous sands occur in abundance in the Iroquois area.

We have, in this vegetal bed, evidence of an interglacial vegetation having spread farther north than the south border of the *morainic* drift, showing that when these moraines were formed, the ice sheet had made an advance and that the moraines cannot be considered mere halting places in the retreat of the ice sheet.

RECENT DISCOVERIES OF ROCK-SALT IN KANSAS. By ROBERT HAY, Assistant, U. S. Geological Survey, Junction City, Kansas.

[ABSTRACT.]

THE examination of salt marshes and salt springs in northern middle Kansas caused Professor Mudge, more than twenty years ago, to suggest that rock salt would be discovered in that region. Wells and borings of considerable depth in the coal measures of eastern Kansas and of the so-called Permo-carboniferous strata have yielded abundant streams of salt water. The discovery of *rock gas* in the eastern counties has recently stimulated speculative drilling in all parts of the state. The prospector's drill at Ellsworth, in August, 1887, struck *rock salt* at a depth of seven hundred and thirty feet. Before the end of the year rock salt had been pierced by the drill at Kingman, Hutchinson, Lyons and Anthony. A previous boring had shown salt shales at Caldwell. The beds of salt—there is more than one at each of these places—vary from twenty to one hundred and forty feet in thickness and they are accompanied by and intercalated with beds of salty shales, and, in one instance, beds of limestone. At

Anthony and Kingman the surface rocks and all the way down to the salt the strata are of Triassic age. At Hutchinson these are overlain by one to two hundred feet of the alluvia of the Arkansas valley. This is also true at Sterling where salt has been reached, July, 1888. At Lyons the red beds (Trias) are also covered with alluvia and possibly some tertiary deposits. At Ellsworth, the Dacotah formations are well developed and the triassic red beds that were there had never before been suspected, for to the northeast the Dacotah formations rest on the Permo-carboniferous, yet in Kingman and Barber counties reduced thicknesses of the Dacotah rest on eroded red beds. But at Ellsworth, the salt is, as elsewhere, at the bottom of the red beds. In the region where the Dacotah is in contact with the Permian, a well marked gypsiferous horizon is found among the Upper Permian beds. In several of the salt borings a gypsiferous horizon is found from sixty to four hundred feet below the lower limit of salt in Permian strata.

The *salt rock* and accompanying saliferous shales form a *saliferous horizon* from three to five hundred feet thick, passing upward from the Permian into the Trias, apparently without break.

At the close of the Permian age, depression of the northeastern area was arrested and uplift commenced while south and west the sinking continued and the saliferous horizon and red beds were deposited all in shallow seas. The northeastern area was depressed to take, on its eroded surface of Permo-carboniferous strata, the earliest Dacotah formations; and the southern and western area was at that time elevated and eroded, becoming again depressed in time to extend the Dacotah area in that direction in some of its upper beds, including the lignite. The axis of oscillation has not yet been made out about which these movements turned, but it would seem that here we have unbroken succession from the Upper Carboniferous to the Dacotah on neighboring areas, through the saliferous and triassic rocks.

In Barber county and to the west and south in the Indian Territory is another gypsiferous horizon in the Upper Triassic strata. It is not impossible that the salt plains of the Cimarron and the salt pool in Meade county may be indications of another saliferous horizon above this.

Kansas is going into the manufacture of salt on a large scale. At Kingman a shaft is being sunk to mine the salt. At Ellsworth, Lyons, Anthony and Sterling works are in progress of which the output will vary from one hundred and fifty to four hundred barrels per day. Hutchinson has already salt blocks in operation whose output reaches 1500 barrels per day, and others in progress which will treble that quantity. It is expected, within a year or two that Kansas salt will be the only salt sold west of the Mississippi river.

THE DISCOVERY OF FOSSIL TRACKS IN THE TRIASSIC OF YORK COUNTY, PA. By ATREUS WANNER, York, Pa.

[ABSTRACT.[1]]

THE author of this paper announced that he had recently found fossil tracks and plants, probably algæ, in the trias of York Co., Pa. A slab from this newly discovered locality of Miameichnites was sent, by Mr. Wanner, to the National Museum in January, 1888.

Professor Hitchcock in commenting on the above paper, stated that he had seen the slab in the National Museum, and that he recognized upon it three species of Dinosaurs, belonging to the genus Anomœpus; also a probable species of Anisopus; all of them closely related to species of those genera long known in Massachusetts. These impressions, he stated, had not been seen south of the Delaware river in New Jersey and Pennsylvania prior to their recent discovery by himself.

He also added that he had a large amount of matter concerning footmarks in his possession, mostly descriptions of new species from Connecticut and New Jersey, which he hoped to make use of in a *Revision of Ichnology*. The time had come for a restatement of the facts of this science, and the investigations into the character of tracks made by living animals which were commenced by the late President Edward Hitchcock, thirty years since, will be continued by his son. The study of the tracks of crustacea was entered upon in January and February, 1888, in Florida. It was found that a considerable similarity exists between the amphipods and isopods and such genera as *Acanthichnus*, *Bifurcalipes*, *Hexapodichnus* and *Climacodichnus* of the Ichnology of Massachusetts. The discovery of *Cheirotherium* from Pennsylvania was announced by the writer in 1868. Additional specimens have since been found at Milford, N. J., now belonging to Lafayette College, Easton, Pa., which show a front foot of ornithic aspect. There seems to be a close relation between this supposed *Cheirotherium* and the *Otozoum* of New England. Other specimens supply needed information about the giant *Polemarchus*.

THE OIL FIELDS OF COLORADO. By Prof. J. S. NEWBERRY, Columbia College, New York.

[ABSTRACT.]

THE only oil field at present productive in Colorado is in the valley of the Arkansas about Florence, a new and growing town, thirty miles above Pueblo. The geology of this region is all Cretaceous. The table-lands bordering the Arkansas valley are composed of the shales and sandstones of the Laramie group which contain valuable seams of coal now largely mined at Coal Creek near Florence for the supply of Pueblo, Denver and the Prairie country toward the east.

The Arkansas river cuts through the Laramie exposing the top of the

[1] The paper will be printed in the Report of the State Geologist of Pennsylvania.

Colorado group; here for the most part dark bituminous shales which have been proved, by boring, to be over 3000 feet in thickness. The oil emanates from the depth of from 1200 to 1500 feet from the surface. About forty wells have been bored within a radius of two or three miles about Florence. Nearly all have yielded oil, but in very different quantity; from two or three up to one hundred and twenty barrels per day. Fourteen wells are now being pumped and the average product is about sixty barrels to the well; so that the present aggregate yield of the wells is about one thousand barrels per day.

The oil is of very excellent quality, light green in color, having a gravity of about 31% Beaumé and has rather an agreeable odor. When distilled it yields 40% of burning fluid and nearly 60% of lubricating oil. The burning fluid is "water white" and has almost nothing of the pungent odor which characterizes our Eastern kerosene. The lubricating oil is of better quality than that furnished by the Pennsylvania wells. It contains so large an amount of paraffine that it becomes pasty at zero temperature. This is a slight objection to its use in the natural state, but is a fault which can be easily corrected whenever the weather becomes cold enough to make it necessary.

The origin of the oil can hardly be a matter of doubt. It is produced by the spontaneous distillation of carbonaceous shales as is the oil of western Pennsylvania, though the geological ages are very different. All the productive territory in the Arkansas oil field is controlled by a syndicate called the Union Oil Company. It owns the land or oil right over more than fifty thousand acres and could apparently increase the production of oil to any desired extent. Now the burning fluid supplies the markets of all the Rocky Mountain region from Montana to Mexico, but the sale of the lubricating oil is prevented by artificial competition. Many thousand barrels are stored and it will ultimately find an outlet by way of the Gulf of Mexico if other channels are closed to it. At present it is only used for fuel under the stills and the boilers of the pumping engines. It is blown into the furnaces with a jet of steam and is a model fuel, but its consumption in this manner is a great sacrifice as it is intrinsically much the most valuable product of the wells.

THE AGE AND CORRELATION OF THE MESOZOIC ROCKS OF THE SERGIPE-ALAGÔAS BASIN OF BRAZIL. By Prof. JOHN C. BRANNER, State Geologist of Arkansas, Little Rock, Ark.

[ABSTRACT.]

IN his "Contributions to the Paleontology of Brazil," Dr. C. A. White describes the mesozoic fossils collected by the author in the Sergipe-Alagôas basin of Brazil. Upon the evidence offered by these fossils Dr. White refers the Sergipe beds to the Cretaceous. He admits, however, that some

of the species have a jurassic aspect, though none of them are identifiable with known jurassic species. Professor Alpheus Hyatt was also struck by the jurassic aspect of some of the cephalopods brought from this region by Professor Hartt in 1869. The author calls attention to the fact that of three beds yielding fossils, and from which collections were most complete, one of them lying at the base of the three, yields most of the fossils of jurassic aspect, an overlying bed yields fossils of cretaceous aspect alone, while the uppermost bed again yields fossils of cretaceous and jurassic aspect.

In referring the beds of this basin to the Cretaceous Dr. White says that while such a conclusion would, he thinks, have been reached from a study of the fossils alone, much reliance has been placed on the corroborative testimony of the geologists of the Brazilian Survey. We have no stratigraphic evidence whatever, throwing light upon the age of these beds. The overlying deposits have been referred to the Tertiary simply because they do overlie these deposits which have been referred to the Cretaceous. Underlying beds have been referred to the Paleozoic, but without any paleontologic evidence.

THE AGE OF THE CRYSTALLINE ROCKS OF ARKANSAS. By Prof. JOHN BRANNER, State Geologist of Arkansas, Little Rock, Ark.

[ABSTRACT.]

GEOLOGICAL maps of the United States represent a small area about Little Rock, Arkansas, and another small area in the southwestern part of the state as Archæan. A recent examination of the crystalline rocks of these and other similar regions in Arkansas shows that none of these rocks are Archæan. They have all been injected into paleozoic strata, and have branching dykes penetrating overlying beds from the principal masses. The exact age of these crystalline rocks has not been determined, for the paleozoic beds into which they were injected have been so completely metamorphosed that they retain no paleontologic evidence of their age. On account of their stratigraphic relations I am inclined to refer most of these sedimentary and metamorphic rocks to the Lower Carboniferous.

THE PERIDOTITES OF PIKE COUNTY, ARKANSAS. By Prof. JOHN C. BRANNER, and R. N. BRACKETT, Little Rock, Ark.

[ABSTRACT.]

DR. OWEN in the second volume of his Geological Reports on Arkansas mentions a small area of igneous rock in Pike county. Geological map-makers appear to have referred this rock to the Archæan. Recent

examinations of the rock and the locality by the senior author show it to have been injected and through lower cretaceous beds. Its mineral composition and structure show it to belong to the new type of peridotites, picrite porphyry, or kimberlite of H. C. Lewis. This is the third occurrence of this rock reported in the United States, the first having been that of Elliott county, Kentucky, described by Mr. Diller; the second that of Syracuse, New York, described by Dr. G. H. Williams. It differs somewhat from the rocks of either of the other localities. It contains no enstatite like the Kentucky rock, its pyroxenic constituent being augite. It contains no ilmenite, but has perofskite in great abundance. It has less fresh olivine than that from Kentucky. There is a total absence of rhombic pyroxene.

THE DISTRIBUTION OF THE GRANITES OF THE NORTHWESTERN STATES AND THEIR GENERAL LITHOLOGIC CHARACTERS. By Prof. C. W. HALL, University of Minnesota, Minneapolis, Minn.

[ABSTRACT.]

THE paper mentions the following as the situation of the granites occurring in Michigan, Wisconsin, Minnesota and Dakota: in Michigan frequently throughout the Marquette and Menominee iron-bearing districts; in Wisconsin in various localities within the so-called Laurentian area of that state, an area which comprises much of the northern central portion with a few outliers in the Huronian and Keweenawan areas; in Minnesota (1) in several belts along the Canadian boundary projecting southwesterly into the state; (2) quite prominent masses, whether connected with those along the boundary or not, around Vermillion, Snowbank and other lakes; (3) the great Mesabi; (4) the exposures of central Minnesota in Stearns, Sherburne, Benton, Morrison and Mille Lacs counties; in Dakota in the southerly and southwesterly portions of the Black Hills.

These granites are either intrusives or granitic veinstones. The veinstones constitute a very insignificant part of these rocks unless it be in the Black Hills where, as Crosby has recently pointed out, there are vast quantities of this material. In Minnesota and Wisconsin granitic veins seldom exceed a few feet in width.

The intrusives which make up the remainder of this great class of rocks have as a group the following general characters:

(1) A coarseness of texture when the Concord or the Barre granite of New England is taken as a type. This frequently passes into a porphyritic structure.

(2) An excessive proportion of the feldspars in their composition; orthoclase, microcline and a plagioclastic feldspar, sometimes oligoclase and sometimes labradorite, are always present.

(3) The presence of quartz in two modifications: (*a*) in segregated areas composed of very large individuals and (*b*) in streams and clusters of minute granules sometimes forming a cement between the quartz, feldspar and other constituents and sometimes saturating these minerals as a secondary product.

(4) The almost universal presence of hornblende as one of the basic constituents and from which the biotite is shown in many instances to be derived.

(5) The presence of a pyroxenic constituent, augite or diallage, in many of the fresher granites situated as a core within the hornblende areas and concluded to be the source of the hornblende which mineral is doubtless wholly secondary.

So far as the question of age can enter into the discussion of the paper it is the writer's opinion that being intrusive the granites have been intruded at various times between the formation of the Laurentian floor of the continent and the close of the agnotozoic era. They as a rock species may be regarded as the product of one of the three or four grand periods of eruptive activity which the intrusive rocks of the northwestern states represent.

On the intensity of earthquakes, with approximate calculations of the energy involved. By Prof. T. C. Mendenhall, of Rose Polytechnic Institute, Terre Haute, Ind.

As an exact science, seismology is in its infancy. Although great progress has been made during the past ten years, and especially in the development of instruments and methods for a more precise study of seismic phenomena, the results thus far have served rather to reveal the complicated nature of the problems involved; and while encouraging the seismologist to renewed effort they warn him that his labors are not to be light. The recent advances of the science have been, and properly, toward the study of the phenomena at hand, the nature and extent of the motion of the earth particle together with the rate at which the disturbance is propagated, in the expectation and hope that in time the location and character of the original cause may be revealed through these.

In the early growth of an exact science one of the obstacles met with is the absence of an exact nomenclature, and seismology furnishes no exception to this rule. Whenever it becomes desirable or necessary to incorporate the meaning of a word in a mathematical expression it is imperative that the necessary restrictions be placed upon its use. It has long been customary to speak of the *intensity* of an earthquake without any special effort to give the word an exact meaning. Generally it is applied to the destructiveness of the disturbance on the earth's surface, and

sometimes to the magnitude of the subterranean cause of the same. But modern seismology proposes to measure the intensity of an earthquake and to express its value numerically. It is worth while, therefore, to inquire in what sense the term may be used with precision and what may be accepted as its mathematical equivalent. Evidently it may mean, and in fact it has been made by different writers to mean, the measure of the surface destruction; the energy per unit area of wave front of a single earthquake wave; the rate at which energy is transmitted across unit of area of a plane parallel to the wave front; and the total energy expended in the production of the original disturbance. The use of well constructed seismographs has furnished us, within a few years, a good deal of fairly reliable information relating to certain elements of earthquake motion, notably the amplitude and period of vibration and the velocity of transmission, by means of which, and aided by a few not very violent assumptions, some of the above quantities may be calculated. They are not identical, numerically or otherwise, and it is manifestly improper to apply the word *intensity* to all of them.

An earthquake wave is generally assumed to be the result of an harmonic vibration, while this supposition is not strictly correct, it is probably not so far erroneous as to materially vitiate the results which follow.

If then,

a = maximum displacement.
t = periodic time.
v_1 = maximum velocity of particle.
V = velocity of wave transmission.
d = density of material through which transmission occurs.

The following are easily obtained:

(1) Maximum velocity, $v_1 = \frac{2\pi a}{t}$.

(2) Maximum acceleration, $\frac{v_1^2}{a} = \frac{4\pi^2 a}{t^2}$.

(3) Energy of unit volume with velocity, $v_1 = \frac{1}{2} d v_1^2 = \frac{2\pi^2 a^2 d}{t^2}$.

(4) Energy of wave per unit area of wave-front $= \frac{2\pi^2 a^2 d V}{t}$.

(5) Energy per second across unit area of plane parallel to wave-front (rate of transmission) $= \frac{2\pi^2 a^2 d V}{t^2}$.

It is well known that Mallet and others of the earlier seismologists attempted to find a mathematical expression which should represent the so-called "intensity" of the shock, by means of the velocity of projection of loose bodies as determined by their range, and also through the dimensions of bodies which would be overturned by the shock. The maximum velocity of the earth might be ascertained by the first method with fair accuracy; the second method is nearly, if not quite worthless in practice, and both are decidedly inferior in design and operation to the modern seismograph which gives the principal elements of the motion directly.

In a paper by Professors Milne and Gray, Philosophical Magazine, Nov.

1881, the following occurs: "The intensity of a shock is evidently best estimated from the maximum velocity of translation produced in a body during an earthquake. This is evidently the element according to which the destructive power is to be measured, it being proportional to the maximum kinetic energy of the bodies on the earth's surface relative to that surface during the shock." Now this statement is inconsistent with that which immediately follows and with their mathematical expression which is

$$I \propto \frac{A}{T^2},$$

equivalent to the second expression given above. This inconsistency was doubtless quickly and first detected by the authors and in a copy of the paper received from them I find interlinear corrections in the paragraph quoted above in virtue of which the words "rate of change of" are substituted for the word "maximum" where it first occurs, and "acceleration" for the words "kinetic energy," thus bringing it into agreement with the remainder of the discussion, and at the same time unquestionably better representing the opinion of the authors, who in all subsequent publications have used the maximum acceleration to represent the intensity as shown in the overturning, shattering and projecting power of the shock.

The same expression, $\frac{v_1^2}{a}$, is used as a measure of intensity by Professor Holden in his paper on "Earthquake intensities in San Francisco,"[1] where he defines it as "intensity of shock defined mechanically = destructive effect = the maximum acceleration due to the impulse." He asserts that "the researches of the Japanese seismologists have abundantly shown that the destruction of building, etc., is proportional to the acceleration produced by the earthquake shock itself, in a mass connected with the earth's surface." This statement is hardly justifiable, at least up to the present time. In the Report of the British Association for 1885, the committee appointed by the association for the purpose of investigating the earthquake phenomena of Japan, consisting of Messrs. Etheridge, Gray and Milne, describe among other seismic experiments one which consisted in determining the quantity to be calculated from an earthquake diagram which would give a measure of the overturning or shattering power of a disturbance. The result of this investigation seemed to show that the acceleration, which by calculation from the dimensions of the columns was necessary for overturning, was somewhere between the mean acceleration, represented by $\frac{4v_1}{t}$ and the maximum acceleration, $\frac{v_1^2}{a}$.

The actual destruction caused by an earthquake wave is undoubtedly a function of many variables, but it seems tolerably certain that maximum acceleration is the leading factor and at the present time no better measure can be found. It appears to me, however, that it is unwise to apply the term "intensity" or "intensity of shock" to this quantity, which might be called the "destructiveness" of the wave, or perhaps, its "destructivity" as indicating a little more clearly the power to destroy.

[1] Am. Journ. Science, Vol. xxxv, page 427.

Dutton and Hayden in their "abstract of the results of the investigation of the Charleston earthquake" presented to the National Academy of Sciences on April 19, 1887, define intensity as the "amount of energy per unit area of wave-front," but in the subsequent discussion use it almost continually as a measure of surface destruction. Upon the first definition they have based a very interesting and novel method for determining the depth of the focus; but in the application of the method to the Charleston earthquake they have used the word in its other and very different sense. A reference to the formulæ given above will show that one of these quantities is inversely as the square of the distance from the origin, as assumed by them in the development of their method, while the other, used in its application, is not so proportional and this must be admitted to be fatal to their deductions.

In the discussion of a somewhat analogous case, Lord Rayleigh says,[1] "the rate at which energy is transmitted across unit of area of a plane parallel to the front of a progressive wave may be regarded as the mechanical measure of the intensity of the radiation." The algebraic expression for this quantity, as shown above, is, of course, similar to that of the quantity last considered, differing from it only in the power of "t" in the denominator. Both are very important expressions; neither is very closely related to "surface destruction" and the latter is unquestionably a suitable measure of the "intensity of an earthquake" in the most important sense.

It thus appears that at least four measures for earthquake intensity are and have been in use, which are expressed mathematically in terms of amplitude, period, velocity of transmission and density of medium in formulæ (1) (2) (4) (5) above. To show more forcibly the necessity for placing some restrictions upon the use of the word, I have compared the "intensities" of two earthquakes, using each of the four expressions. The disturbances compared are those of May 6 and May 11, 1884, at Tokyo, Japan, the observations being made by Professor Milne.[2] The same instrument, located in the same place, was used in both and the interval of time between the two is so small as to forbid any important change in the conditions. That of May 6 is called "A" and that of May 11 "B." The results are as follows:

	(1)	(2)	(4)	(5)
$\frac{B}{A}$ —	1.1	1.7	.9	1.3

from which it is evident that much depends on the measure of intensity adopted.

As stated in the beginning of this paper, the more recent work of seismologists has been in the study of individual disturbances for the purpose of determining the principal elements of motion, amplitude, period, direction and speed of transmission. In this study much has been learned. From the nature of the case we are almost absolutely restricted to an investigation of surface phenomena and we are soon forced to admit that

[1] Lord Rayleigh, Theory of Sound, Vol. II, page 16.
[2] Trans. Seis. Soc. Japan, Vol. X, page 27.

what goes on at the surface cannot accurately represent what is going on below. Among other reasons for this conclusion we have notably the greatly varying results obtained from the same disturbance at points comparatively very near to each other. The amplitude at one point may be two or three times that at another a few hundred feet away and not only this but the periodic times do not agree, and when the measure of maximum acceleration is applied to the disturbance, its so-called intensity or destructiveness will vary greatly within a small area. As a matter of fact it has long been known that such variations in destructive power do occur in nearly all earthquakes. Not only do the above elements vary, but the speed of transmission, when once the surface is reached, is undoubtedly not constant, although we have no reason to believe that it is not approximately so in the rocks through which it is, in the main, transmitted. Most of these irregularities are doubtless due to the non-elastic character of the materials lying near the surface and to their lack of homogeneity. In spite of their appearance in the phenomena of the surface it is difficult, if not impossible, to believe that they exist in the rocks below. It is more reasonable to assume that during an earthquake the waves of transmission are, in the main, and until the surface is reached, somewhat regular in their form and approximately constant in certain of their elements. It may also be assumed that in amplitude and periodic time the subterranean wave, although doubtless much less than the surface wave, cannot differ from it enormously, so that elements of motion obtained by seismometric observations upon the surface, may be applied within certain limits to the investigation of the energy involved, the results being considered as rough approximations.

On these assumptions the following calculations have been made:

Let A be the area of a portion of the wave-front and l a length measured at right angles to A. Then formula (5) above, which shows the energy per second across unit area, multiplied by $\frac{Al}{V}$ will evidently express the energy required to generate the waves existing at any moment in the volume lA. That is

$$\begin{aligned} &\frac{2\pi^2a^2dV}{t^2}\cdot\frac{Al}{V} \\ &= \frac{2\pi^2a^2d\,Al}{t^2} \\ &= \frac{2\pi^2a^2}{t^2}\cdot m \quad (m = \text{mass of volume } lA) \\ &= \tfrac{1}{2}mv_1^{\,2} \end{aligned}$$

That is to say, the work consumed in generating waves of harmonic type is the same as would be required to give the maximum velocity to the whole mass through which the waves extend.[1] Sir William Thomson, who was probably the first to apply this principle, in his calculation of the mechanical value of a cubic mile of sunlight, concludes that in the case of a complex radiation this value is more likely to reach twice that of the above expression.

[1] Lord Rayleigh, Theory of Sound, Vol. II, page 17. Sir William Thomson on "The Possible Density of the Luminiferous Medium."

On the assumption that the maximum velocity of the particle is known we may now apply this formula to the calculation of the energy involved in an earthquake. For this purpose I have selected first, the Japanese earthquake of January 15, 1887, which disturbed over thirty thousand square miles of territory and the elements of which were well recorded on the Tokyo seismographs. Assuming a mass of 150 lbs. per cubic foot and taking a cubic mile as the volume to be considered, I find that to put it in vibration required the expenditure of 2,500,000,000 ft. lbs. of energy, and this might be called the "mechanical value of a cubic mile of earthquake." Assuming that an area of one hundred miles square with a mean depth of one mile was thus in vibration at any one instant of time, which is not improbable considering the known rate of transmission and the long duration of the earthquake, the amount of energy thus represented would be 25×10^{12} ft. lbs. This energy might be generated by the fall, under the action of gravity, of a cube of rock one thousand feet on each edge, the mass of which would be 75,000,000 tons, through a vertical distance of about 166 feet.

It would be interesting to apply this method to the Charleston earthquake of August 31, 1886. Unfortunately no seismographic records were made and the elements of motion are largely matters of conjecture. Messrs. Dutton and Hayden in the report already referred to express the opinion that in some localities the displacement must have been as much as ten inches or one foot. This seems to me improbable, but it may be safe to say that over a considerable area it was as much as one inch. Nothing is known with certainty as to the period of the oscillations, but as it generally increases with the magnitude of the disturbance it would probably not be grossly incorrect to call it two seconds. Assuming these magnitudes I find the energy of a cubic mile of the Charleston earthquake, taken near enough to the epicentrum to be disturbed as above, to be equal to 24,000,000,000 ft. lbs. The speed of transmission of this disturbance has been pretty well determined by Newcomb and Dutton, to be approximately three miles per second, so that a cubic mile would be disturbed in one-third of a second. To do this would require 130,000,000 horse power. Assuming as before that an area about the epicentrum one hundred miles square was thus disturbed the energy involved would be 24×10^{13} ft. lbs., and the rate of its expenditure would be that of 1,300,000,000,000 horse power.

All of these numbers can only be regarded as gross approximations. They probably indicate the order of magnitudes involved and may be useful until more reliable data are furnished.

AN ANCIENT CHANNEL OF THE OHIO AT CINCINNATI. By Prof. JOSEPH F. JAMES, Agricultural College, Maryland.

[ABSTRACT.]

CINCINNATI is built upon a terrace of the Ohio river as have also been many other cities of the western states. The Ohio is one of the streams which has cut for itself, in part of its course, a new channel. Part of the old bed is now known as Mill Creek, a small stream emptying into the Ohio below Cincinnati. It is an insignificant stream, occupying a channel excavated by a much larger river. The bed of the ancient river is of great importance to Cincinnati, inasmuch as by its means many railroads have access to the city. Without this valley, tunnels or inclined planes would have been necessary to enable the railroads to enter the city.

The surrounding hills are of the solid blue limestone of Lower Silurian age, quarried so extensively for building stone and for lime. The hills were once continuous across the river to the mouth of the Licking from Price's Hill, but the Ohio has forced a passage through them. The edges of Mill Creek valley are of rock, but the bottom is sand, gravel and clay. Far beneath lies the bed rock. At the foot of George street, in Cincinnati, the drift is forty-eight feet thick; at Cumminsville, the rock is sixty feet below low water in the Ohio. In another case, one hundred and twenty feet were penetrated before bed rock was reached. At Ivorydale, ninety-eight feet of gravel overlie the rock. At Hamilton, twenty-five miles north of Cincinnati, the gravel is two hundred and fourteen feet thick. Thus, the rock here is ninety-one feet below low water in the Ohio river, so that there is a descent in the rocky bottom of Mill Creek from Cincinnati to Hamilton *below* the ground, while at the surface Hamilton is one hundred and twenty-three feet higher than Cincinnati.

In consequence of the barrier across its channel at Cincinnati, the Ohio once followed the present valley of Mill Creek north to Hamilton, at which point it was joined by the Big Miami. Together the streams flowed southwest and entered the present channel of the Ohio at Lawrenceburg.

At the east end of Cincinnati is the wide mouth of the Little Miami river. For a distance of two and a half miles no rock is exposed at the surface, though the whole country is covered by huge banks of drift fifty and one hundred feet high. Where this drift now is, was the ancient channel of the Ohio, and if followed up this channel is found to unite with the western arms of the Ohio north of Cincinnati near Ludlow Grove. To still further prove the existence of this ancient channel is the fact of the bed rock being exposed in the channel of the Ohio at and below the mouth of Mill Creek, while above this point it is covered by sixty feet and more of gravel and sand.

After the glacial period the river forced its way over or through the barrier which had previously existed below Mill Creek and cut a new channel, the present one, for itself. It found its previous channel choked up with débris and had to make for itself a new but not so extensive a channel as the old one.

NOTES ON THE ORIGIN AND HISTORY OF THE GREAT LAKES OF NORTH AMERICA. By Prof. J. W. SPENCER, University of Georgia, Athens, Ga.

[ABSTRACT.]

Discovery of the ancient course of the St. Lawrence River.

PREVIOUS investigations by the author showed that there was a former river draining the Erie basin and flowing into the extreme western end of Lake Ontario, and thence to the east of Oswego, but no further traceable, as the lake bottom rose to the northeast. Upon the southern side there was a series of escarpments (some now submerged) with vertical cliffs facing the old channel. By recent studies of the elevated beaches, it is demonstrated that the disappearance of this valley is due to subsequent warpings of the earth's crust, and that the valley of the St. Lawrence was one with that of Lake Ontario. Recent discoveries of a deep channel, upon the northern side of Lake Ontario (a few miles east of Toronto), and of the absence of rocks to a great depth under the drift, far beneath the surface of Lake Huron, between Lake Ontario and the Georgian bay,— and in front of the Niagara escarpment, between these lakes,— of a channel in Georgia bay, at the foot of the escarpment, and of the channel across Lake Huron, also at the foot of a high submerged escarpment, show that the ancient St. Lawrence during a period of high continental elevation rose in Lake Michigan, flowed across Lake Huron and down Georgian bay and a channel, now filled with drift, to Lake Ontario; thence by the present St. Lawrence valley to the sea—receiving on its way the ancient drainage of the Erie basin and other valleys.

Origin of the basins of the Great Lakes.

The two questions involved are the "origin of the valleys" and the "cause of their being closed into water basins." The basins of Lakes Ontario and Huron are taken for consideration. The previous paper, upon the course of the ancient St. Lawrence, shows that the Huron and Ontario basins are sections of the former great St. Lawrence valley, which was bounded, especially upon the southern side, by high and precipitous escarpments, some of which are submerged. Upon its northern side there were lesser vertical escarpments, now submerged, with walls facing the old valley. The valley was excavated when the continent was at a high altitude, for the eastern portion stood at least 1,200 feet higher than at present, as shown by the channels in the Lower St. Lawrence, in Hudson's straits and the New York and Chesapeake bays. The valley was obstructed in part by drift and in part by a north and northeastward differential elevation of the earth's surface, due to terrestrial movements. The measurable amount of warping defied investigation until recently, but it is now measured by the uplift of the beaches and sea cliffs. Only one other explanation of the origin of the basins need be considered — that of the "Erosion by Glaciers," (*a*) because the lake basins occur in glaciated re-

gions; (*b*) glaciers are considered (by some) to erode; (*c*) supposed necessity, as the terrestrial warping was not known.

In reply: living glaciers abrade but do not erode hard rocks, and both modern and extinct glaciers are known to have flowed over even loose moraines and gravels. Again, even although glaciers were capable of great plowing action, they did not affect the lake valleys, as the glaciation of the surface rocks shows the movement to have been at angles (from 15° to 90°) to the trend of the vertical escarpments against which the movement occurred. Also the vertical faces of the escarpments are not smoothed off as are the faces of the Alpine valleys, down which the glaciers have passed. Lastly, the warping of the earth's surface in the lake region, since the beach episode, after the deposit of the drift proper, is nearly enough to account for all rocky barriers which obstruct the old valley and form lake basins.

Establishment and dismemberment of Lake Warren.

This is the first chapter in the history of the great lakes and is subsequent to the deposit of the upper boulder clay, and therefore the lakes are all very new in point of geological time. By the warping movements of the earth's crust, as shown in the beaches — after the deposit of the later boulder clay — the lake region was reduced to sea level and there were no Canadian highlands northward of the great lakes. During the subsequent elevations of the continent, beaches were made around the rising islands. Thus between Lakes Erie, Huron and Ontario, a true beach was formed at 1690 feet above the sea, around a small island rising 30 feet higher. With the rising of the continent, lake (or perhaps gulf of) Warren — a name given to the sheet of water covering the basin of all the great lakes—was formed. A succession of beaches of this lake have been worked out in Canada, and from Lake Michigan to New York, extending over many hundreds — almost thousands —of miles. Everywhere the differential uplift has increased from almost zero, about the western end of the Erie basin, to three, five, and, in the higher beaches, more feet per mile. With the successive elevations of the land this lake became dismembered, as described in the succeeding papers — and the present lakes had their birth. The idea that these beaches in Ohio and Michigan were held in by glacial dams to the northward is disproven by the occurrence of open water and beaches to the north, which belong to the same series, and by the fact that outlets existed where glacial dams would be required.

Discovery of the outlet of Huron-Michigan-Superior Lake into Lake Ontario, by the Trent Valley.

With the continental elevation described in the last paper—owing to the land rising more rapidly to the northeast—Lake Warren became dismembered and Huron, Michigan and Superior formed one lake; the Erie basin was lifted out of the bed of Lake Warren and became drained, and Ontario remained a lake at a lower level. The outlet of the upper lake was

southeast of Georgian bay by way of the Trent valley into Lake Ontario, at about sixty miles west of the present outlet of this lake. The outlet of this upper lake was 26 feet deep where it connected with the Trent valley, and the channel was from one to two miles wide. This, for a few miles, is cut across a drift ridge to a depth of 500 feet. With the continued continental uplift to the northeast (which has raised the old beach at the outlet, into the Trent valley, about 300 feet above the present surface of Lake Huron), the waters were backed southward and overflowed into the Erie basin, thus making the Erie outlet of the upper lakes to be of recent date. This is proven by the fact that the beach, which marked the old surface plain of the upper great lake, descends to the present water level at the southern end of Lake Huron.

Erie the youngest of all The Great Lakes.

The Erie basin is very shallow, and upon the dismemberment of Lake Warren was drained by the newly constructed Niagara river (except perhaps a small lakelet southeast of Long point). Subsequently the northeastward warping (very much less in amount than farther northward at the Trent outlet) eventually lifted up a rocky barrier and formed Erie into a lake in recent times, thus making Erie the youngest of all the lakes. The beaches about Cleveland are not those of separated Lake Erie, but belong to the older and original Lake Warren.

NOTE.—To distinguish from the modern, the ancient valley of the St. Lawrence, above described, is named the "Laurentian;" the ancient river from the Erie basin, the Erigan; the Huron-Michigan-Superior lake, the Algonquin, as also the beach which marked its shores, and the river which discharged its waters by the Trent valley. The expanded, but separate, Lake Ontario is named the Iroquois, as also its principal beach, now at 116 feet above its modern surface, at the extreme western end of the lake, while at about 135 miles northeastward (near Trenton) its elevation is 435 feet.

REMARKS ON AN UNDESCRIBED VEGETABLE ORGANISM FROM THE FORT UNION GROUP OF MONTANA. By Prof. LESTER F. WARD, U. S. Geological Survey, Washington, D. C.

[ABSTRACT.]

THE specimens were collected in 1883 at two points forty miles apart and on opposite sides of the Yellowstone in the vicinity of Glendive, Montana. At one of the localities, viz., Iron Bluff, twelve miles above that town and on the right bank of the river some fifteen meters above low water mark, it occurs in a red burned cliff in light arenaceous clay, the specimens being very large, sometimes measuring thirty centimeters across. At the other locality, namely Burns' Ranch, twenty-eight miles below Glen-

dive on the left bank of the river and close to the water's edge, the specimens are smaller and more perfect, being embedded in a fine grained bluish clay shale. The fossil shows a large circular center 2–7cm. in diameter from which there proceed in radial arrangement in all directions a large number of single flexible stems varying in length and having a width of 2–3 mm. These horizontal, radiating stems are sometimes slightly sinuous, lying upon and often crossing one another. They exhibit for most of their length, beginning near their attachment to the center, a row of toothlike appendages on each side, which are about ½mm. in width at the base and about 1mm. apart, obliquely ovate in shape with obtuse tips and always projecting forward toward the distal end of the stem at an angle of 50°. Close examination with a high power reveals the presence of a continuous epidermal membrane composed of hexagonal cells connecting these projections. The stems are uniform in width and not marked by any longitudinal costa until within some 3cm. of the apex when they expand into an elongated elliptic blade, or head, terminating the stem. Through the center of each of these heads run two rows of what appear to be spore-cases, one on each side of the median line and separated from each other by a very narrow interval forming a continuous groove running longitudinally through the head. The supposed spore-cases are somewhat elongated transversely, and arranged in pairs, filling most of the surface of the head, but leaving a winged expansion at the central or widest portion. They average 1½mm. in length and ½mm. in thickness. Below the base of the head, where the teeth begin, they appear to cease, but in most cases a careful inspection reveals their presence in an obsolete and probably functionless form, and this is sometimes distinguishable for considerable distance down the stem where it assumes more the aspect of a series of articulations widening as the center is approached. The lateral teeth of the stems usually cease below the head, but cases occur in which they continue for some distance along the margins of the expanded lamina.

Considerable, but not as yet exhaustive, search has been made through the literature of paleontology to find the analogues and determine the affinities of this singular organism, as yet almost entirely without success, the forms figured under the names Discophorites, Gyrophyllites and Tænidium, perhaps having the greatest resemblance, but not close enough to indicate any relationship. Specimens have been shown and sent to a number of eminent paleobotanists and paleontologists, but none, so far as heard from, can give any idea of its nature. Its vegetable character having been questioned it has also been shown or sent to the best authorities on invertebrate zoology in this country and in Europe. Those who have thus far expressed an opinion have uniformly denied its relation to any known animal. It is remarkable that it is the botanists who have suggested its possible animal nature, the zoologists inclining to regard it as a plant.

Having collected and long studied the specimens, superintended their delineation, and compared them with a great number of forms both fossil and living, I may be permitted to advance in as few words as possible, the

theory which I provisionally hold as to the nature of this organism, as follows: I am disposed to regard it as a "comprehensive type" of vascular cryptogamic life, embodying some of the characters of several well known living types, viz., 1. The large tufted central base is suggestive of that of most species of Isoetes, and the long weak stems of certain of these species are observed to recline and lie prostrate in all directions around this center. 2. The double row of spore-cases at the apex of the stem agrees in all essential respects with that of Ophioglossum, and the elliptic expansion may be regarded as homologues of the larger blade-like fronds of that genus, which may easily be imagined to have the spores borne along its median line instead of on a special fruiting frond. 3. The prostrate sinuous habit is not widely unlike that of certain creeping species of Lycopodium, as, e. g., *L. annotinum*, and the toothlike appendages may be the reduced homologues of the scale-like leaves of that genus. 4. A still further approach is seen in Selaginella where the scales have become distichous and the stems flat and closely creeping. This parallel is well-nigh complete in those species, such as *S. Douglasii*, in which the spores are borne in terminal spikes, like those of most Lycopodiums, except that these are more or less flattened and two-ranked. 5. Finally, ignoring the appendicular organs of Marsilia we see in the fruit-bearing portion a further analogy to our fossil, the fruiting stems radiating from the thickened base and bearing the spores at their apex.[1]

The fossil would thus represent a highly generalized type and may be phylogenetically related to all these more specialized modern forms with each of which it seems to possess some characters in common.

The paper was illustrated by lantern views of the original fossils and of carefully prepared drawings.

THE PALEONTOLOGIC HISTORY OF THE GENUS PLATANUS.[2] By Prof. LESTER F. WARD, U. S. Geological Survey, Washington, D. C.

[ABSTRACT.]

THE genus Platanus is one of those waning types, like Ginkgo, Sequoia, Liriodendron, etc., of which so much has been said of late, and though now constituting an entire order and containing only seven species, it is evidently the descendant of a large family embracing a number of genera, each with a fair representation of specific forms. Twenty extinct species of Platanus are now recorded from Tertiary and Cretaceous strata, which are, however, in most cases founded on the impressions of leaves. But in

[1] Since the date at which this paper was read communications have been received from the Marquis Saporta and from Dr. A. G. Nathorst, both of whom agree that the organism is probably a cryptogamic plant related to Ophioglossum.

[2] Published in full in the Proceedings of the United States National Museum, Washington, 1888, Vol. XI, pp. 39–42, pls. xvii–xxii.

addition to these there is a large number of aberrant types resembling Platanus in many respects, though also resembling some other living genera, which have been variously referred to Sassafras, Aralia, Liquidambar, Liriodendron and to the supposed extinct genera Araliopsis, Aspidiophyllum, Protophyllum and Credneria.

The object of this paper was to point out the probable genetic relationship among all these extinct types and to show that they are the probable ancestors of the modern genus Platanus. Special emphasis was laid upon the significance of the basilar lobes of the form described by the author as *Platanus basilobata* from the Fort Union group, and upon the connection between this and similar appendages sometimes found on leaves of *P. occidentalis*, and of certain fossil species of Platanus, Aralia, and Aspidiophyllum. The supposed Sassafras leaves of the Dakota group were also discussed with a view to showing that they are also archaic platanoid types, and it was argued that the nervation of these leaves differs in some fundamental respects from that of the leaves of the only living species of that genus, the normal form of which was shown to be entire and not trilobate, as is commonly supposed.

Finally, it was contended that, inasmuch as five of the seven living and a large preponderance of the fossil species of Platanus, as well as most of the ancestral forms referred to, are American, and as the type is found in this country in very much older strata than in the Old World, it must be conceded that, notwithstanding the historic antiquity of the oriental plane tree in connection with the early development of the human race in Asia and Europe, nevertheles it is, paleontologically, an American tree, and had its origin in this country.

This paper was illustrated by lantern views of fifteen of the forms selected to exhibit the phylogenetic development of the genus.

EVIDENCE THAT LAKE CHEYENNE CONTINUED TILL THE ICE AGE. By Prof. J. E. TODD, Tabor, Iowa.

[ABSTRACT.]

THIS paper called attention to several facts hitherto unpublished which indicate that eastern Nebraska, western Iowa and southeast Dakota were occupied by a fresh water lake when the drift first began to be deposited in that region. The facts and considerations are as follows:

1. An extensive deposit of fine sand containing a few fossil bones, overlain in some places by a lead-colored clay without pebbles, and some fossiliferous silt resembling loess, is found occupying much of the region, especially at lower levels. Ten localities were mentioned where these formations have been observed, the more notable being Fairview, Dak., Mills Co., Iowa, and Lancaster Co., Neb. A large fossil claw of some gigantic

mammal[1] was shown which was obtained from Mills Co., Iowa, in the sand below the drift.

2. The occurrence of a stratum of volcanic ashes in such position as to show that wide areas were occupied by still water, just preceding the deposition of the drifts in some parts and during it in others. The localities described and pictured were in Knox Co., Neb., and near West Point, Neb.

3. An objection which may be urged, from the depth of the channel of the Missouri in this region, is removed by several facts which go to show that said channel has been wholly excavated since the glacial epoch.

(*a*) The rock under the present bed is unglaciated and unoccupied by drift deposits, as has been recently demonstrated by observations made in sinking the piers of bridges at Blair and Omaha.

(*b*) The Missouri is still deepening its trough with every great flood. This has been determined by soundings at such times.

This fresh water lake, from its time and location, may be quite confidently considered a portion of the great body of water which occupied the western plains during late Tertiary times, and which was named by King, Lake Cheyenne.

THE TERRACES OF THE MISSOURI. By Prof. J. E. TODD, Tabor, Iowa.

[ABSTRACT.]

THIS paper embodies the observations of the writer while connected with the U. S. Geol. Survey, during the last six years. They extend from the 41° to the 46° of latitude.

The terraces are found to be readily arranged in three groups, viz.: The Silt Terraces, which rise from 5 to 175 feet above the present stream, the Lower Bouldery terraces, from 70 to 350 feet, both these being much higher toward the north, and the Higher Bouldery Terrace, which has been less definitely and extensively traced, rising from 350 to 475 feet. The development, location and structure of the different terraces were discussed in detail.

All decline in height down stream. All contain coarse material, boulders and gravel, but this is especially prominent in the bouldery terraces, and in all this diminishes in amount down stream. Even the bouldery terraces are commonly capped with silt several feet in depth.

The lower silt terraces were not considered; the highest one of this class was observed at Pierre to be 160–125 feet above the river; near Crow Creek Agency, 150–110; at Chamberlain, 125; ten miles above Wheeler, 85; Wheeler, 105–85; Fort Randall, 80–45; and at Niabrara, about 25 feet above the river.

[1] Since the above was written this has been determined by Professor Leidy to be a claw of *Megalonyx*.

The lower bouldery terraces, two of which may be traced above Wheeler, are by far the most distinctly marked and extensively developed. The upper one shades, on the one hand, into the adjacent upland and, on the other, gradually subsides into the lower terrace of this group. This is especially seen above the Bijou Hills at Le Beau and about Chamberlain. The upper seems to mark about the surface of the former stream, while the lower sometimes corresponds to the bottom of ancient channels, one of the more remarkable of these, for example, extending southwest from American Crow Creek past Red Lake to the Missouri.

From the vicinity of the mouth of Pratt Creek, both terraces rapidly decline down stream to a lower level which is reached near Yankton Agency, where they seem to coalesce and from that point they are indistinguishable. This is attended with a widening of the terrace between Yankton Agency and Choteau Creek, to a breadth of three or four miles.

The following table sufficiently shows these facts.

Heights on the lower bouldery terraces above the river.

LOCATION.	LOWER.	HIGHER.
La Grace	240–250	
Le Beau	200	300
Mouth of Little Cheyenne		250–270
Pierre		275–325
Crow Creek Agency	200–250	320
Two miles above Chamberlain		325
Five miles below Chamberlain	200	300
Little above Bijou Hills	100–175	
Near mouth of Snake Creek	150	330
Near mouth of Pratt Creek	90–126	300–350
Ten miles above Wheeler		200–250
Wheeler		135–175
Yankton Agency	130–240	
Niobrara	200	
Yankton	120–145	
Sioux City	115–150	
Blair	90–110	
Bellevue	50–70	

Traces of this terrace, more or less conspicuous, are found on most, if not all tributaries of the Missouri outside of the Second or Gary Moraine. It has been specially noted, by the writer, along White, Niobrara, James, Big Sioux, Elkhorn, Little Sioux, Boyer and Platte rivers. One-half to three-fourths the height of this terrace is usually composed of older rocks, and the upper portion of drift, sometimes stratified, but often very closely resembling till, with 10–15 feet of silt usually capping the whole. Boulders are often abundant, especially in the vicinity of the moraine, and where the terrace slopes rapidly, as near the mouth of Pratt Creek.

The higher bouldery terraces are least perfectly developed. Their age,

height above the river, and the probable lacustrine character of much of the river at the time of their formation accounts for this.

There seems to have been but one, except near the Bijou Hills. Its height above the river does not vary greatly from 400 feet; and at the mouth of the Niobrara seems to become continuous with the upper or western limit of the drift in Nebraska.

A comprehensive view of the data given leads to the following conclusions:

1. The southward sloping of the terraces does not demand, though it would favor the idea that there had been recent northward elevation.

2. The abrupt descent of the lower bouldery terraces below the Bijou Hills indicates that the Missouri has recently cut through the divide between the White and Niobrara rivers, as was suggested by the writer in 1884 (See Proc. A A. A. S., 1884).

3. Because the lower bouldery terrace extends up the James river, Pratt Creek, Okobojo, Little Cheyenne, etc., through the first moraine to the gaps of the second moraine, it is correlated in time with the occupation of the second or Gary moraine, while less confidently the higher bouldery terrace is similarly correlated with the first or Altamont moraine.

Systematic results of a field study of the Archæan rocks of the Northwest. By Prof. Alexander Winchell, Ann Arbor, Mich.

[ABSTRACT.]

Two entire seasons spent by the writer in the study of the Archæan rocks of Minnesota, Wisconsin, Michigan and Canada, in addition to more casual studies during many years previously, have resulted in a view which seems clear, simple and conclusive, respecting the grand divisions existing, and their equivalences at remote points. In the typical Huronian region of Canada, we find two massive quartzite terranes underlain by blackish, siliceous argillites—the "slate conglomerate" of Logan and, at bottom, another and more vitreous quartzite. Intersecting these are vast dikes of diabases, often slaty and in some places of such extent as to appear interbedded sheets. The "Animike" formation of Thunder Bay and of northeastern Minnesota is continuous with the siliceous argillites, and lithologically identical, but with accessions of flinty and jaspery beds, and especially in Minnesota of vast deposits of magnetitic schist. Underneath this system, and in marked discordance of stratification with it, is a system of argillitic sericitic and chloritic slates, which embrace the hæmatites of the Marquette, Gogebic and Vermilion regions. Unquestionably this system is not Huronian, though generally so reputed. Still lower, but stratigraphically comformable, occur crystalline schists, followed by vast beds of gneisses. The crystalline schists ally themselves

with the gneisses and graduate into them. All these present the characters ascribed to the Laurentian. But it must be said that true unbedded granites are seldom if ever seen within the range of my observations. We have then the three following systems of Archæan rocks:

III. A system equivalent to Huronian.
II. A system unnamed (Marquettian.)
I. A system equivalent to Laurentian.

THE USE OF FOSSILS IN DETERMINING THE AGE OF GEOLOGIC TERRANES. By Prof. H. S. WILLIAMS, Ithaca, N. Y.

[ABSTRACT.]

THE study of the data and the methods employed in comparing the Russian Devonian (as described in a paper by P. N. Veunkoff) with the Devonian of the Eifel and Ardenne regions of northern Europe (as discussed by M. Achile Six in a paper "on the Russian Devonian" in Annales de la Société geologique du Nord, XIV, p. 67-126, 1887) and these with the Devonian system in America, gives me the occasion to formulate and discuss the following proposition:

Whenever the attempt is made to determine the relative geologic age of separate terranes the following rules are believed to be generally applicable.

1. It is not the more conspicuous and fixed characters of fossil species which furnish the most valuable data for determining age, but the slight and generally more or less plastic characters.

2. It is the degree of prevalence and abundance, and not the mere presence or absence of a particular temporal modification of a species that constitutes the most reliable indication of the age of a terrane.

3. Faunas taken as wholes are more valuable than individual species—and the fauna thus considered includes the elements of *relative abundance* of the component species, and takes account of the relation which the *epochal facies* of each species bears to its typical form, and the *temporal place* the species occupies in the history of its genus.

THE FOSSIL WOOD AND LIGNITES OF THE POTOMAC FORMATION. By F. H. KNOWLTON, U. S. National Museum, Washington, D. C.

[ABSTRACT.]

PERHAPS no American geological formation, which has been made the subject of recent investigation, has given rise to more extensive discussion

or has furnished more valuable scientific results, than has the Potomac Formation. First clearly differentiated by Prof. Wm. B. Rogers, as long ago as 1840, it has during the past decade, and more particularly during the last three years, been made a special study by Messrs. McGee, Fontaine, Ward and Marsh, and at the present time the history of its deposition and abundant animal and plant life, is better known than is the history of many of the European formations with which it has usually been correlated. Its exact stratigraphic position, however, is still unsettled, although strong presumptive evidence is at hand. It was called by Rogers, the Jurasso-cretaceous or upper secondary sandstone. In 1885, Mr. W. J. McGee, arguing from the then available paleo-botanical evidence, considered it to be "Lower Cretaceous in age—the American equivalent of the European Neocomian." Prof. Wm. M. Fontaine, who has so thoroughly worked up the plant impressions, regards it as Wealden, while Prof. O. C. Marsh, who has studied the numerous vertebrate remains, claims for it a Jurassic age.

The Potomac Formation is remarkable for containing the oldest dicotyledonous flora yet discovered. Of the three hundred and sixty-five species of plants described by Professor Fontaine, no less than seventy-five species are dicotyledons. They do not consist of the highly differentiated genera and species which characterize the other dicotyledonous floras, such as the Dakota group, but are new and strangely archaic in appearance.

My own studies of the Potomac Flora have been extensively confined to an investigation of the internal structure of the fossil wood, which is very abundant in this formation. It occurs under two widely different conditions, viz.: as lignite and as silicified wood. Both these forms, as indeed all the plant remains, occur principally in lenticular pockets of hard, bluish clay, which pockets bear evidence of having been transported *en masse* from the original beds in which they were laid down. There is almost no transition between the two forms, although there is evidence that some of the silicified forms are also represented in a lignitized state; that is owing to different conditions of fossilization, some specimens of a species were silicified, while others were turned to lignite.

In color the lignite is almost uniformly jet black. It has a specific gravity of about 1.383, and breaks with a true conchoidal fracture like ordinary anthracite. When thus broken, it does not exhibit superficially the slightest trace of organic structure. It may, however, be split along certain lines, notably in the direction of the medullary rays, when very plain structure shows superficially. The method of examining this lignite, was that recommended by Griffith and Henfrey in their Micrographic Dictionary (2d edition, p. 178), for the examination of coal. When prepared this way, the sections are tolerably transparent, and the structure is easily made out. The most casual examination shows that this material has been subjected to great pressure, which has so entirely crushed and distorted the cellular elements, that it is difficult to recognize the original form. The examination of a large series of sections serves to give a pretty correct general idea of it. In transverse section, the lumen of the cells is seen to be almost entirely closed up, the result of lateral pressure.

In regard to the identification of this lignite, it is manifestly impossible to attempt more than an indication of its general character and position. That it is coniferous, is beyond question. The absence of wood elements other than tracheïds, which were provided in some cases at least, with bordered pits, and the number and arrangement of the medullary rays, make the coniferous nature clear. From the abundance of the genus Cupressinoxylon, in the Potomac Formation, as shown by the silicified examples, it is probable that most of the lignite may be also of this genus, particularly as there is in many cases, a marked resemblance, so far as I am able to interpret the distorted structure, between it and some of the species described from silicified specimens. Also several species undoubtedly entered into the composition of this lignite.

The silicified material was examined by the methods usually employed in the study of petrographic material, viz.: by cutting thin sections and mounting in Canada balsam. This material is all coniferous. It belongs to two well-known genera, Cupressinoxylon and Araucarioxylon.

Cupressinoxylon, as now accepted, is a somewhat comprehensive genus and is usually regarded as representing in a fossil state, the wood of Sequoia, or a nearly allied form. This view is strikingly confirmed in the present instance, as Professor Fontaine has described, from cone and leaf impressions, no less than twelve species of Sequoia, and typical cones of Sequoia have been found at Beltsville, Md., associated with the lignite and silicified wood. Four species of Cupressinoxylon have been detected from the Potomac Formation, and although possessing affinities with the other described forms, they nevertheless differ from them in important features, and have all been regarded as new to science. They have been named as follows: *C. pulchellum*, *C. McGeei*, *C. Wardi*, and *C. Columbianum*.

The genus Araucarioxylon represents the wood of the Araucarian pines in a fossil state. The single species is described as new under the name of *Araucarioxylon Virginianum*.

THE GLACIAL BOUNDARY IN SOUTHEASTERN DAKOTA. By Prof. G. FREDERICK WRIGHT, Oberlin, Ohio.

[ABSTRACT.]

THE object of this paper was to present the result of some observations made during June and July of the present season in the vicinity of the Missouri river between Yankton and Fort Yates in Dakota. The country traversed is in part that reported upon by Professor J. E. Todd at the Philadelphia meeting of this association in 1884 (see Proc., pp. 381–393).

After giving a summary of the facts relating to the character of the glacial deposits near the boundary of ice action in the eastern and central states, the speaker proceeded to say: The surprising thing to a glacialist

upon a first visit to southeastern Dakota is the extent of the apparently level area where the till comes to the surface. This impression is heightened probably by the absence of forests, and would very likely be the same in portions of Ohio and Indiana were it not for the timber. James river valley in Dakota is depressed in the centre about five hundred feet below the edge, but it is, roughly speaking, seventy miles across; so that the slope does not strike the eye. So level is the country that every special line of glacial accumulation is a prominent feature in the landscape, and the various halting-places of the ice in its retreat are readily discerned, and were accurately delineated, so far as my observations have extended, by Professor Todd in the paper referred to. For a long time a lobe of ice filled the James river valley, running parallel with that occupying the upper Minnesota valley, and extending southward into Iowa. The edges of these lobes thinned out along the north and south line which runs near the east margin of southern Dakota, and favored the accumulation of the lines of hills known as the "Coteau des Prairies." Professor Todd and others speak of this as a series of "terminal moraines" formed along the sides of the reëntrant angle whose apex penetrated to the vicinity of the Sisseton Agency. I am not sure but that it would facilitate an understanding of the subject to speak of the Coteau des Prairies as a *medial* moraine toward which the glacial debris (carried upon the deeper portions of the ice) gravitated in both directions. But certain it is, that, starting from a point at the Sisseton Agency different lines of glacial accumulations stretch southward at varying angles,—the later accumulations forming the more obtuse angle. Coming up the valley of the James from Yankton one crosses the oldest of these accumulations (the Altamont moraine) in the neighborhood of the city itself, and the second (or Gary moraine) in the neighborhood of Mitchell, sixty miles to the north, having run parallel with it, however, for about thirty miles. The third, or Antelope moraine, is encountered near Huron, about sixty miles north of Mitchell, and continues visible upon either side of the river, about twenty miles distant, beyond Aberdeen.

On the western side of this lobe there are corresponding lines of accumulation, the outer of which is the vicinity of the Missouri river and on its eastern side. Everywhere in coming up from the Missouri on to the plateau. which is in most places from four hundred to five hundred feet above the river, one encounters two or three terraces covered with boulders, the two highest of which are about three hundred and four hundred feet respectively. These moraines rise out from this general level one hundred or two hundred feet higher, and are marked features of the landscape. The streams entering the Missouri from the east are all of them short, the longest not being more than forty miles in length. These streams are in all cases that I observed marked by broad and elevated local terraces, the edges of which where they overlook the immediate trough of the stream are crowded with granitic boulders. In some cases, as Professor Todd has shown, these valleys terminate abruptly at the water-parting, as if being the continuation of glacial streams from the east which had originated upon the ice lobe while it filled the James valley. The boulders

which are superabundant over all this region are largely of granite or gneiss, and must have been transported over a long distance, in some cases probably more than four hundred miles, from the northeast.

Owing to the difficulty of penetrating the Indian reservation, Professor Todd had not been able to extend his observations to any great extent west of the river and north of Pierre. But through the courtesy of Rev. Thomas L. Riggs, and of his assistant, Rev. James F. Cross, missionaries to the Sioux, I was permitted to enter the reservation at Fort Yates, and traverse with them, on the west side of the river, the region intervening between Fort Yates and Oahe, in the vicinity of Pierre, making the distance travelled over about 150 miles.[1] Leaving Fort Yates, and going four miles in a south-southwesterly direction, we reached an elevation of 425 feet, all of which was made during the last mile, and represents the general height of bluffs on either side of the trough of the Missouri, which is here about four miles wide. The elevation of Fort Yates above tide is 1694 feet, making the elevation of the bluff 2119. The fort is situated on a glacial terrace about fifty feet above the present high-water mark. Eight miles farther on, the elevation was 2294 feet, which is about the general level for many miles. The elevation of Grand River was 1819 feet. The divide between the Grand river and the Moreau is almost exactly the same elevation (namely, 2294 feet) with that between the Grand and Fort Yates. South of the Moreau river the divide between that and the Cheyenne rises to 2404 feet. Over the whole of this distance from Fort Yates, boulders of granite and gneiss were very abundant, being as numerous upon the higher as upon the lower points. Frequently the boulders were several feet in diameter. But, two or three miles to the south of the highest land between the Moreau and the Cheyenne rivers this northern drift suddenly ceased at an elevation of 2354 feet, or fifty feet lower than the previous reading. From this point south and east to Oahe (a distance of about sixty miles) no signs of northern drift were seen, except in the valley of the Cheyenne river, upon coming down to a level of 2100 feet. The elevation of the higher land between the Cheyenne and the Bad river was from 2200 to 2300 feet. Upon coming again into the valley of the Missouri, northern drift began to appear immediately upon the western bluff, at about 2100 feet above tide.

The western limit of drift upon the Northern Pacific railroad is fixed by Professor Chamberlin at Sim's Station, about forty miles west of Bismarck. Prof. N. H. Winchell, in his report upon the Custer expedition to the Black Hills, in the summer of 1874 (see pp. 22 and 23) found the limit of drift to be in the Dog Teeth Buttes, thirty or forty miles south of Sim's Station, and about twenty miles west of the 101st meridian. Putting these observations with my own, it would appear that the limit of

[1] As I had no barometer with me, the elevations given were determined by Rev. Mr. Riggs upon a subsequent journey, and forwarded to me. Being the result of single readings, they are of course liable to a considerable margin of error, which, however, may be eliminated in subsequent journeys. But I am confident that the margin of error is not large, since the readings are nearly correct at the river crossings.

glacial drift between the Northern Pacific railroad and Pierre runs from Sim's Station southward, crossing in a pretty direct line the valleys of the Cannonball, the Grand, and the Moreau river, to the divide between the Moreau and the Cheyenne, where it is certainly ten or twelve miles west of the 101st meridian. At this point it is running nearly east and west between the Moreau and the Cheyenne; but just where it strikes the trough of the Missouri I am at present unable to tell. It certainly cannot be much west of the mouth of the Cheyenne.

Over the glaciated portions traversed, I was unable to find any typical deposits of till, but this may have been owing to the absence of favorable places for observation, since the rolling prairie region afforded few opportunities for erosion. Frequently, however, knolls and short ridges projected above the general level, and were literally covered with boulders, pebbles, and gravel, all of which appeared to be more or less washed. Nor was I able to find on any of the boulders distinct marks of glacial scratching. About halfway between the Grand and Moreau rivers, upon the very summit, we traversed an extensive shallow depression resembling a kettle-hole about two miles wide. This was bordered by hills about one hundred feet above the bottom. The rim was marked by numerous knobs which were perfectly covered with granitic and gneissoid boulders. Many large boulders were also scattered over the bottom, these being frequently from three to four feet in diameter. The boulders seem to be limited to a superficial deposit. On approaching the Moreau, while still upon the high land, numerous boulders from four to eight feet in diameter were observed.

On striking into the valley of the Cheyenne river, about twelve miles above its mouth, and coming down upon a terrace, about three hundred feet below the general level, a few small gneissoid boulders appeared. Again on crossing the river at about the same elevation, we struck into an old river-bed, which seemed to be perfectly level, and about two miles wide, and to extend as far as the eye could reach both up and down the valley. Granitic pebbles were abundant in this deposit and they were all well waterworn, and evidently occupied a depth of a number of feet. Here and there over it were scattered a number of small granitic or gneissoid boulders a foot or more in diameter. Whether the source of this granitic material was the glaciated region to the north or not, I am at present unable to tell, since the Cheyenne has access to granite in the Black Hills where it rises. Still, from the similarity of the boulders to those found on the highlands to the north, I think a connection will be found between this old river-bed and the glaciated region to the north. I am inclined to believe that this is the line of marginal drainage by which the water (which both before and since occupied the trough of the Missouri) worked around in front of the ice, meeting the Missouri valley near the mouth of the Cheyenne river. Since my return, Mr. Riggs has crossed the Cheyenne about six miles farther up stream, and reports this old river-bed as there on the north side, and coming in from the north at an angle of about twenty degrees. I shall look for further evidence of this stream having worked around Fox Ridge from the glaciated region where we left it on the divide between the Moreau and the Cheyenne rivers.

About ten miles south of Grand river, however, we found Long Butte to be not like the buttes which had occurred at frequent intervals in previous parts of the journey, capped with sandstone, but to be a gravel deposit evidently an old river bottom containing much material brought down from the Black Hills, and all well waterworn. This ridge extended for some miles, being a very marked feature in the landscape, and is, I suppose, a remnant left by the erosive forces which had operated for a long time upon the underlying cretaceous formations, wearing them away faster than they did the superincumbent gravel deposit.

The further study of this portion of the glacial border, will be important for its bearing upon current theories concerning the glacial period. The thinness of the deposit, and the washed appearance of the material of which it is composed, will be interpreted by some to indicate that it is the eroded and wasted margin of a first glacial period, separated by an enormous lapse of time from the last glacial period. This, however, does not seem to me to be necessarily the case. In interpreting the marginal deposits of the glacial period, we should remember that the motion of a glacier constantly diminishes towards the margin until it finally reaches a line of stability; so that the deposits at the very margin are those which have been carried upon the ice, rather than shoved along under it, which would account for comparative absence of abrasion. Furthermore, the marginal deposits are, of course, the older deposits on any theory, and have been longest subjected to the action of erosive and disintegrating agencies. Thirdly, in the present case, these marginal deposits were peculiarly subject to erosive action from the vast drainage of the Missouri river which had been pushed out of its natural channel. These considerations seem to me still to leave it an open question, so far as these deposits are concerned, whether they do not belong to an earlier stage of the same epoch during which the heavier deposits had been made upon the east side of the river. At any rate, it would seem that the abruptness of the termination of these deposits, shows that they were due to direct glacial action, rather than to that of floating ice. It would be difficult to conceive of a subsidence in that region which should have produced a body of water with a shore corresponding to this drift limit. Now that the country is likely soon to be opened to settlement, we shall look to investigations in that quarter for much light upon many problems of glacial theory.

SOME THOUGHTS ON ERUPTIVE ROCKS WITH SPECIAL REFERENCE TO THOSE OF MINNESOTA. By Prof. N. H. WINCHELL, State Geologist of Minnesota, Minneapolis, Minn.

GEOLOGISTS know of but two sources for all the rocks which constitute the crust of the earth, viz.: (1) cooling from fusion, and (2) solidifying after being disintegrated and distributed by water. Under accepted hypothesis the former were first to be formed, and the latter have resulted by a long continued series of dissolution and selection through the agency of oceanic water, from the former.

Those that have cooled from fusion, known as igneous, or eruptive, rocks have been divided broadly into *basic* and *acid* eruptives. Though it is obvious that this distinction may fade out when subjected to universal and rigorous application, yet for the purpose of this paper it may be accepted in advance, since these terms do afford a classification which covers a multitude of facts and sufficiently characterize the two evident great divisions of eruptives.

In offering to the Section a few thoughts on these two classes of eruptives I wish to disclaim any desire to ignore the views and facts that have been published by others. I shall simply base some statements on some facts which have come under my own observation, to which I have given considerable reflection during the past ten years, derived from my work on the geological survey of Minnesota, referring to my reports for details of the evidence. Time has not yet been given to make an exhaustive study of similar facts from other parts of the world, and the tentative hypotheses that I shall give may have to be modified or abandoned on making wider comparisons.

The *basic eruptives* are sometimes distinguished as *volcanic*, in distinction from the acid which at the same time have have been called plutonic. They are such as are known commonly to have flowed in recent times from volcanic vents, and to have been spread in extensive sheets and streams over the face of the earth. They are heavy, dark-colored, often bearing iron-peroxide (and in rare cases metallic iron): as a group they are dolerytes,[1] and embrace diabase, basalt, some of the diorytes, and those rocks that in Wisconsin and Minnesota have been designated gabbro. They contain less than sixty per cent of silica.

The *acid eruptives* (so called) are such as have been supposed to have constituted the earliest super-crust of the earth, and are now exposed at the surface because they have been kept uncovered by denudation. They occupy, where seen on the continental areas, the lowest of the rock strata; and from these ancient bosses are seen various offshoots that penetrate in forms of veins, sheets and dikes, the rocks immediately overlying. In the higher rock horizons this class of eruptives is comparatively rare, but isolated small areas are known in Europe and America. In most instances, however, in which they are found in Silurian, Carboniferous or Tertiary time, they seem to exhibit a diversity of minor characters of structure, by which they are distinguishable, though their chemical composition, in its essential ingredient of silica, shows their general alliance to this class of eruptives. The acid crystalline rocks embrace granite, syenite, apparently some diorytes, felsyte, trachyte, quartz-porphyry and all such as have a silica content of sixty per cent or more.

In addition to the distinction of content of silica these two classes exhibit other differences, when compared in their original and unweathered state, and when not modified by immediate contact with other rocks. The basic rocks are *dark colored*, gray, varying to hornblendic-brown or chloritic-green. The acidic as a class are of *lighter colors*, as required by the

[1] Dana: *Am. Jour. Sci.*, Nov., 1878.

higher per cent of silica, red, pink, gray, olive-gray, bluish-gray and grayish-white. They are often porphyritic with quartz and with orthoclase. They differ also in weight, the basic rocks having specific gravity from 2.65 to 3.5, and the acidic from 2.4 to 2.7. In general the specific gravity of the basic may be stated at 3, and of the acid at 2½.

One of the most noticeable differences, however, between the basic and the acid rocks, is in their *manner of occurrence and their relative amounts, among the other strata*. The basic eruptives have poured out from the interior of the earth in vast floods, covering the surface for thousands of square miles. When solidified they present a homogeneity of character and composition that demonstrates the vastness of the source from which they came, such as can be explained only by referring it to the molten, original interior of the earth. All the attendant phenomena of fissures filled by molten rock, profound heating and changing of the preëxisting strata, and the basaltiform structures that result from slow cooling, attend these wonderful eruptive sheets. On the other hand, the acid eruptives, when they possess the undoubted characteristics of a former fluidity—*i. e.* if they fill fissures in rocks that preëxisted, and are separated from the basal (Laurentian) gneisses—are found to be of small amount and scattered very arbitrarily and haphazard among the other strata. They are not known (in Minnesota) to have been spread out in sheets like the basic dolerytes, although some of the felsytes are in sheets that are embraced in the stratification of the terrane with which they are associated. They rise in isolated knobs, here and there, unconformably overlain by later sedimentary strata, or they pass by slow changes, when traceable, into crystalline masses that appear to belong to the Laurentian gneisses. In the territories the trachytes form isolated buttes that are thrust upward in the Tertiary and Cretaceous strata. They exist as laccolites rather than overflow sheets.[1]

The two classes of eruptives are *differently distributed in time and rock horizon*. The acid rocks form the fundamental gneisses of the Laurentian, constituting rock masses many thousands of feet in thickness. Subsequent to that time they dwindle in amount, and exhibit a diversified character, but break out again with considerable abundance in Tertiary time. The basic eruptives appertaining to the age of the Laurentian gneiss are so rare that they may be considered wanting, or at least at the present time undiscovered. They appear in the later crystalline terranes, and they seem to have increased in amount in Silurian time, to have prevailed widely in Mesozoic time, and to have culminated in the Post Tertiary.

If we seek for *similarities* between these two classes, we find that they both show evidence of former plasticity, have been mechanically transposed from one position to another among the rocky strata, have each produced a metamorphic effect on the contiguous rocks, have cooled and congealed, taking a more or less perfectly crystalline texture, and by entering the fissures have recemented the strata when they have been rent by upheaval, thus constituting dikes and elvans.

[1]G. K. Gilbert: The Geology of the Henry Mountains, 1877.

These contrasts indicate different sources, or methods of generation, and it is the purpose of this paper to call attention to a plausible hypothesis which assigns to each a genesis which is consistent with a great number of facts.

The basic eruptive is of *unquestioned deep-seated origin*. It has flowed from the interior of the earth at great epochal intervals, and is known to flow from volcanoes at the present time. But throughout its history it has maintained a constancy of character. The dolerytes of the Post-Tertiary are not distinguishable from those of the Cretaceous and Jura-Trias, nor from those of the Cambrian. The diabase of the Taconic, when fresh, is not different from some of the finer dolerytes of the Tertiary. The so-called gabbro of the Mesabi, in Minnesota, is found to graduate into a rock which is diabasic. The peridotytes of the Kawasachong eruptive are also diabasic. An earlier probably eruptive rock (the Vermilion outflow,—the mica-hornblende rocks which I have styled the *Vermilion group*) while evidently changed from its original condition, yet exhibits the essential elements of a basic eruptive, and in some places becomes a basic dioryte.

It being admitted that the basic eruptive has risen from a deep source through the fissures that have been formed in the crust of the earth, it remains to find a reasonable explanation for the occurrence of acid eruptive rock.

It is difficult to understand how from the same volcanic vent shall flow, from the same source, successive floods of basic and acid rock. In those few instances in which such a phenomenon has been asserted it may safely be affirmed, still, that the acid rock is not known to have come from the same depth as the basic, and hence it is allowable to suppose that it came from a less depth. This being granted, there is no fact known to the writer which conflicts with the following general principle, which I shall take as the *theme* to be discussed.

All acid eruptive rocks result from the hydro-thermal fusion of preëxisting sedimentary strata embraced in the super-crust of the earth.

(1) The "fundamental gneiss" which in America is admitted to be the *Laurentian gneiss*, has been supposed to have constituted the original crust of the earth, which resulted from the congelation of the surface of the globe. But *a priori* it seems that this is highly improbable, if not impossible, from all that we know of the nature of that molten mass. This gneiss is an acid rock. The molten rock that above is admitted to have come from below the crust of the earth is basic. If the nature of the rock which formed the first film of rigid matter over the primeval earth be called to mind, we have no authority for making it an acid film. *It was a basic film of dark pitchstone.* It was formed, shattered and reformed. It was broken and washed by the mists and waves of the infant ocean. It was dissolved by the hungry fresh waters of the perennial rains. It was buried under its own ruins. As it became able, in scattered small spots, to constitute rigid land, it was washed by the ocean and again dissolved. About its shores accumulated siliceous beaches. About these beaches, as they emerged, other

beaches were formed. Then, by some disturbance, they were all buried. Then they slowly emerged again, and the land grew by further sandy accretions. The ocean's waters seized all the soluble ingredients, and the soluble ingredients were more numerous, since the waters were hot, than they are at the ocean level at the present day. The insoluble were left on the shores. The land grew through the Laurentian age, with vicissitudes of upheaval, solution, submergence; and emergence, solution and siliceous accumulation. The continents were in embryonic condition, but they were *necessarily of acid rock*. Locally, and perhaps sometimes generally, this siliceous sedimentation was invaded by overflow of basic eruptions from below the crust. But as the crust was thin, the stress would be slight, and the fractures were of small moment, giving rise to but inconsiderable outflows. They seem to have been lost, during the Laurentian age, either by resolution by the ocean and the distribution of the insoluble residue along the shores, or by later hydro-thermal metamorphism. As the sediments increased in thickness, the alkalies and alkaline earths were steadily abstracted by the powerful solvent and destructive action of the ocean. This process once begun, would result, if continued through geological time, *in the formation of acid continental areas*. But as the ocean became more and more alkaline it necessarily reached the point of saturation, and began to make rocks, off shore, of another sort, producing limestones and magnesian limestones, and to furnish conditions fit for the display of low animal life. The palæozoic limestones resulted. Further additions to the continental areas brought these limestones, and all later rocks, to the level of the dry land, while at the same time burying under deeper and deeper loads of superincumbent rock material, the sediments of the Laurentian ocean.

(2) The sediments of the Laurentian age, therefore, being necessarily siliceous, when acted on by the well-known forces of heat and pressure undergo the consolidation and crystallization which is attributed to these agents. *They constitute the acid "fundamental gneiss."* They are not only consolidated, but within the broad expanses of the later continental history they are subjected to that hydro-thermal re-fusion which is now admitted to pervade not only the original igneous matter of the earth's crust but also the strata of the super-crust. In this plastic condition they are ready to enter all the avenues that open for the escape or release of plastic matter under pressure; and they have made their appearance at many places and at all times in the geological history of the earth. The variability of these acid eruptives in composition, color, and degree of crystalline structure, together with the smallness of their geographic area, and comparatively limited amounts, go to indicate not only a variable source but a comparatively limited supply. As contrasted with the constancy of composition and vast floods that characterize the basic, this is one of the most marked and important differences.

(3). *The manner of eruption of the acid rock is frequently, if not generally, in the form of laccolites, instead of surface overflows.* Considering the greater specific gravity of the basic eruptive, it has been remarked by Mr. G. K.

Gilbert that it is a remarkable fact that in the territories of the United States, the basic eruptive has, so far as known, always been ejected at the surface, while the lighter acid, so far as known, has never, or rarely been so ejected in any considerable amount, but has insinuated itself laterally, within the strata of the superincumbent rocks, before reaching the surface, forming the *laccolite*, which Mr. Gilbert was the first to describe (*Geology of the Henry mountains*). To what extent this manner of eruption may characterize the acid eruptive bosses of the Cupriferous and of the Taconic, is unknown; but many of the facts that are connected with these earlier acid bosses in Minnesota, are indicative of such an origin. Now this singular fact, that the lighter eruptives have been able to rend apart the strata through which they were rising on their way to the surface and insinuate themselves horizontally among them, and that the heavier have found rest only by ascending to the surface, is one of those that point most unmistakably to the correctness of the principle above stated.

In the light of late theoretical considerations, the deep-seated strata of the super-crust of the earth, are in a state of hydro-thermal fusion. This condition of fusion is one that allows a more or less complete re-arrangement of the chemical and mineral constituents, although it does not necessitate any profound and wide-spread transposition of those elements from place to place. This condition of plasticity becomes one of fluidity on the removal of the superincumbent pressure. The depth at which this plasticity exists varies with the pressure exerted, and with the degree of heat.

This condition of the super-crust being admitted it needs only to be united with the before-mentioned acid nature of the super-crust, increasing in acidity towards the surface, to bring the plastic mass under those hydrostatic conditions, which are exemplified by the presence of two liquids of different density in the same enclosure. The denser one will seek its equilibrium, and will find its place at the bottom. The lighter one will rise to the level of its equal specific gravity in the surrounding mass. If there be a complication, such as results from the graduation of the lighter into the heavier, through different stages of weight and plasticity, the lighter or more plastic may rise in great blebs through the overlying heavier or less plastic. Such blebs, which may be considered the laccolites of Mr. Gilbert, would rise to that point where they found either sufficient adhesive power in the rocks to resist their further ascent, or an equal specific gravity in the super-crust. There they would rest, after an accumulation of sufficient amount to reëstablish a general equilibrium of pressure.

Mr. Gilbert's statement that such laccolites in the Henry mountains were formed between the sedimentary strata without the creation of fissures in the sedimentary beds, implies, as he argues, that all those beds were under great pressure, and were so affected by hydrothermal forces that they had a semi-plastic condition and were "stretched" to conform with a solid covering, to the fluid intrusion. It would only require that this semi-plastic condition should increase at somewhat greater depth, so as to become a fluid one, with some change in the pressure, or some increase of heat, such as was likely to occur at an epoch of upheaval, to

institute a bodily transposition of portions of the plastic mass so as to adjust themselves to the changed relative specific gravity. *Such flows could never reach the surface.* The plastic condition gives place to a rigid one before the surface is reached, and, if it did not, the super-crust becomes, *a priori*, more and more acidic and hence of a lighter specific gravity toward the surface. Such intruded masses therefore must occur only as laccolites.

It may be asked, Why do not the basic eruptives operate in the same way, and form laccolites? So far as I can see this can be answered only by supposing that they have issued through fissures in the crust of the earth, that have opened down to the molten interior, and that the ducts through which they have passed have been rigid instead of plastic. It is obvious, however, that such a vent would first exhaust the plastic acid layer of sedimentary rock before the basic could obtain access to the vent. Such escape of acid eruptive material would give rise first to a nest of laccolites, raising the whole surrounding region considerably above the average level, and subsequently such acid nuclei would form the central portions of the mountains or mountain range. If the eruption continued the ejections at last would become of a basic character, and some such seem to have been continued from the earliest geological time to the present.

Again, it may be asked, How can the eruption of trachitic, or acid, rock at the surface, be explained, in those instances where it is known at the present time to form more or less extensive hill ranges? It may be answered that where such trachitic hills do exist they may be only the uncovered laccolite of Mr. Gilbert, such as has lately been discovered in the case of Bear butte, one of the trachitic hills that are found near the Black Hills in Dakota. The sedimentary rocks of the Tertiary, as well as of the Cretaceous are rapidly removed by sub-aërial erosion. Hundreds of feet thickness of these strata have been carried away by erosion over hundreds, and even thousands of square miles, in western Dakota. The present condition of this process of removal is witnessed in the well-known "bad lands" which occur at various places in the western territories. Such removal over extensive tracts has made old laccolitic intrusions (as the Bear butte and Inyan Kara) appear as if they had been at first sub-aërial extrusions. Again it might be answered that if acid superficial extrusions have occurred in considerable amount, as seems by the descriptions of some European geologists, such extrusions were produced by more sudden fracturing and displacement of the whole super-crust, locally, *by mechanical forces*, so as to allow, or to compel, the underlying plastic mass of sedimentary material to flow out at the surface of the earth. But it need scarcely be stated that such an occurrence is not governed by the principles that have been appealed to show the necessarily laccolitic character of a normal acid eruption. Such accidents doubtless have occurred and have allowed the extrusion of softer portions of the super-crust.

To re-capitulate, then (and there is but one idea in this paper), we find the following principle sustained by physical and chemical laws, and not

at variance with any known geologic facts, viz.: *all acid eruptives are the result of the hydro-thermal fusion of siliceous sediments belonging to the super crust, and all basic eruptives are derived from below the super-crust of the earth.*

If, secondly, we make an attempt to apply this principle to the super-crust and to ascertain what order of sequence there has been in the outflow of basic eruptive material, we shall be able to identify the great epochs of molten, basic eruption by a casual examination of the strata. They have marked characters, and no geologist can mistake them except when they have themselves been subjected, as in the Archæan, to the same hydro-thermal re-fusion and change, or when they have been so modified by atmospheric agents that they are no longer massive but schistose, or have taken silica into their interstices, so that they do not or may not give a basic aggregate percentage of silica.

Guided by these evident characters, I wish to call your attention to the four great epochs of basic eruption which I think are found in the lowest rocks of Minnesota.

(1) *The Vermilion group*, so named in the fifteenth report of the Minnesota survey, embraces what have been styled frequently the *crystalline* schists of that region. They lie next above the Laurentian gneisses. Their manner of contact on the gneiss is remarkable. It is characterized sometimes by several alternations of gneiss with dark crystalline schist, in distinct layers from ten to fifty feet thick, evincing a gradual, or interrupted, transition from one to the other, as if of sedimentary nature. The dark element in these schists is mica or hornblende. At a somewhat higher level in the terrane there is great confusion, and the gneissic rock is mixed confusedly with the schist, in the form of mutually penetrating dikes and isolated patches, which seem to imply a plastic condition for both. But occasionally the dark rock exists in enormously greater quantities and has almost wholly a massive structure. At the same time it becomes almost wholly hornblendic, the micaceous ingredient being visible only on exposed knobs and weathered surfaces; also in the fissures and accompanying the jointage planes. There is unmistakable evidence, which cannot be detailed here, that the mica, which characterizes the *Vermilion group*, is wholly the result of change from hornblende, which seems to have been, or is now at least, the basic mineral of the greater hills and the massive portions. This horizon marks the earliest known appearance, in Minnesota, of basic material in the super-crust of the earth in such amount as to constitute a terrane worthy of special designation. The upper side of these crystalline schists reveals a quiet sedimentary transition into the next formation — that named Kewatin (as limited by the fifteenth report) by Mr. A. C. Lawson of the Canadian survey. There are many alternations of acidic sediments with basic, beautifully arranged in conformable stratification. In this part of the Vermilion group the rocks are characteristically hard mica schists at first, of dark color, but become graywackes as the micaceous element fades out. It is very easy to refer this mica to a changed condition of grains of hornblende derived abundantly from the disintegration

of the basic eruptive just preceding. It is also very easy to explain its gradual disappearance, as the sediments passed into the graywackes, becoming more acidic by reference to the necessary progressive silicification of the super-crust already mentioned.

(2) The next eruption of basic materials succeeds the graywackes. It has not received any systematic name, but forms the falls of Kawasachong, in Minnesota. It is not so prominently a hill-making rock, but often appears as chloritic schists, seeking more retired portions of the topography. It is also of a lighter color, and sometimes embraces grains of free quartz which seem to be indigenous. It is prominently conglomeritic, or perhaps more correctly agglomeritic, with masses of rock like itself, though containing also, in some places, abundance of jaspilyte in fragments, as well as much chalcedonic silica in small pebbles and minute grains. This rock is prevalent at the iron mines at Tower, at Ely, and at Negaunee, and is styled, when stained by iron, "paint rock," and when not stained, soapstone and greenstone. The eruptive origin of this rock at Negaunee has been shown by Dr. M. E. Wadsworth. It stretches from Tower eastward to Ely and to Ogishke Muncie lake. It was later in date than the origin of the hematites of the formation, and seems to have terminated the Kewatin formation. It is supposed to be the equivalent of the "Serpentine group" of Dr. C. Rominger, a remarkable old eruptive of the Marquette region of Michigan. The contact of this eruptive on the graywackes about Tower is most remarkable, but cannot be described here. The graywackes and argillytes are wonderfully broken and thrust together again, and the eruptive rock seems to embrace them in the same manner as mentioned of the basic and gneissic rocks at the bottom of the *Vermilion group*.

This horizon of disturbance has been illustrated by numerous figures in the fifteenth report of the survey.

(3) We have knowledge of no formation, in the northwest, intervening between this eruptive and the unconformable overlying Animikie or Huronian. But there may have been later sedimentary deposits that are hid by this unconformable overlap. Whether there were or not, the next great epoch of basic eruption followed immediately after the Animikie, and is well described as the *gabbro or Mesabi overflow*. This is by all considerations the most notable eruptive terrane in Minnesota. The rock, while usually a well characterized gabbro, such as has been described by the Minnesota reports, and by the late Prof. R. D. Irving of the U. S. Geological Survey, varies to diabase and to dioritic rock, and apparently to a red-weathering diabasic rock. The minerals of this gabbro exhibit great variations in their relative amounts. At Duluth and at Iron lake, back of Grand Marais, the titanic magnetite so prevails that the mass of the rock, in some limited portions, is regarded as iron ore. In the region of Little Saganaga lake and in Carlton's peak the labradorite element is almost the sole ingredient; and in numerous instances, but in limited areas, about Birch lake, the mineral olivine is almost the only one present. This eruption seems to have flowed from numerous vents that occurred along the region where it is now found in greatest amount. It was so copious that

the basaltic covering which it forms is one of the most conspicuous parts of the visible geology within much of the region of the Animike. It lies not only on the slates and quartzytes of the nearly horizontal Animike, but also extended northward so far as to lie over the graywackes, schists, and older eruptives of the Kewatin. More than that, it also flowed on northward over the granitic acid rocks of the Laurentian and conceals them from sight in considerable areas, a circumstance which seems to have induced some Canadian geologists to classify it as "Upper Laurentian." This eruptive is considered to be identically the same in characters and age as the "hypersthene-rock" of Emmons which constitutes the main mass of Adirondacks in New York.

(4) The fourth, and latest, basic eruptive rock known in Minnesota is that which is seen in the great *Cupriferous formation.* That this is of much later date than the gabbro, although it lies immediately on the gabbro, is shown by the singular pudding-stones seen near Beaver bay where large transported boulders of the light-colored "labradorite rock" of Carlton's peak are embraced in a dark matrix of doleryte belonging to this eruption. Opinion is divided yet as to the age of the copper-bearing rocks of the northwest. If they are of the age of the Potsdam sandstone, these eruptives which are interstratified with the sediments, constitute the only known instance of a general outflow at that horizon and age. If they be of Mesozoic age they have their parallels in a general eruption which is found distributed from Connecticut and New Jersey to the western territories. It is the purpose of this paper only to call attention to them, as the latest in the Northwest, and not to inquire into their precise age.

THE OCCURRENCE OF CHALK IN THE NORTH AMERICAN CRETACEOUS. By ROBERT T. HILL, U. S. Geol. Survey, Washington, D. C.

EVIDENCE THAT THE MOHAWK RIVER AT A VERY REMOTE PERIOD CHANGED ITS CHANNEL OF DRAINAGE. By Dr. A. S. TIFFANY, Davenport, Iowa.

EXTRA-MORAINIC STRIÆ IN THE MISSOURI VALLEY. By Prof. J. E. TODD, Tabor, Iowa.

THE ARCHIMEDES LIMESTONES AND ASSOCIATED ROCKS IN NORTHWESTERN ARKANSAS. By Prof. FREDERICK W. SYMONDS, Arkansas Geol. Survey, Fayetteville, Ark.

GEOLOGICAL HISTORY OF THE OZARK UP-LIFT. By Prof. G. C. BROADHEAD, Pleasant Hill, Mo.

ON A NEW METHOD OF CONSTRUCTING GEOLOGICAL MAPS. By JAMES T. B. IVES, F. G. S., Toronto, Canada.

SOME PHYSIOGRAPHIC NOTES ON NORTHEASTERN MINNESOTA. By Prof. C. W. HALL, University of Minn., Minneapolis, Minn.

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NOTES ON THE PRE-GLACIAL DRAINAGE OF WESTERN PENNSYLVANIA. By P. MAX FASHAY, Beaver Falls, Pa.

IVORYDALE WELL IN MILL CREEK VALLEY, OHIO. By Prof. JOS. F. JAMES, Agricultural College, Maryland.

A NEW GAS WELL AT CLEVELAND, OHIO. By H. P. CUSHING, Cleveland, Ohio.

GEOLOGY OF CLEVELAND, OHIO. By H. P. CUSHING, Cleveland, Ohio.

THE CLEVELAND SHALE AND ITS FOSSIL FISHES. By Prof. J. S. NEWBERRY, Columbia College, New York, N. Y.

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ADDRESS

BY

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VICE PRESIDENT, SECTION F.

ON THE CAUSES OF VARIATION IN ORGANIC FORMS.

LADIES AND GENTLEMEN:

BUT few alternatives are left your vice president in choosing a subject for this annual occasion. With the modern world-wide activity in biology, to give a review, however condensed, of the progress and discovery of the year, would require far more time than can be given by one who yields to the pleasure and fascination of original research what little time can be snatched from the slavery of official routine and administration. Any such general review, if attempted, could not be critical or authoritative beyond the limits of one's own specialty, and the task is now well done by the combined efforts of a number whose work we get in the German and English records.

> "One science only will our genius fit,
> So vast is art, so narrow human wit."

To give a similar review of progress in one's own specialty would be to interest but a limited number. I have decided, therefore, to give expression to a few thoughts which have occupied my mind for many years, on the laws of biologic evolution, and particularly on the causes of variation in organic forms. I have been helped to this decision by the appearance, since last we met, of the "Life and Letters" of the immortal Darwin and the loss from among us of the mortal presence of our own beloved Gray. Both men, in their work, gave eminent illustration of the fact that the subjects which

I thus take for text may be approached and elucidated from quite different roadways or beginnings. These subjects are, moreover, of still absorbing interest; for we recognize that in evolution is locked up the origin of man, just as in it are involved his present and his future, subjects which from time immemorial, or from the earliest infancy of the race, have deeply concerned him. The demonstration of the fact, the establishment of the law of organic evolution, have given fresh impetus to inquiry. To know something definite of the past but intensifies our desire to read in that past some prophecy of the future.

Evolution, as a fundamental principle in organic nature, is to-day more thoroughly established and proved in the mind of the average biologist than the Copernican theory itself.

From the time when Aristotle prophetically remarked that "Nature passes so gradually from inanimate to animate things, that from very continuity the boundary between is indistinct," through the suggestive writings of Lamarck, Erasmus Darwin, Oken, Goethe, Baden-Powell, Geoffroy-St.Hilaire, Wells, Chambers, Lyell and Spencer, the belief in biologic evolution was growing and expanding, until it ultimately blossomed in the simultaneous labors of Darwin and Wallace.

Prior to the writings of these last it was necessary to fight for the general principle, and long after the appearance of the "Origin of Species" there was a violent clash between the advocates of special Creation and those of Evolution; so that much as Darwin felt and cared for natural selection he was in the beginning more interested in settling the question of "Creation vs. Modification." The fight has continued with ever-waning forces on the part of the creationists,[1] until to-day the evolutionists are victorious and no longer under the necessity of marshalling the facts to bring conviction to the skeptical. Thus biology, the last stronghold of the creationists, has been brought into correspondence with chemistry and physics, in which the opponents of evolution had long before given way. The clearing of the smoke on this last battlefield is comparatively so recent that, in the popular mind, evolution yet applies only to the organic kingdom, and it is in this more limited sense that we, as biologists, particularly use the word, and in which I shall use it for the most part in this address. We depend for

[1] The term is used in the theological sense of creation by supra-natural means. Evolution is also creation, but by natural process.

evidence on an immense number of well ascertained facts, so that the proofs of organic are often stronger than of inorganic evolution. Phylogeny, or the geologic succession of life, as proved by palæontology, finds its correspondence in Ontogeny, or the developmental history of the individual, particularly in its embryonic phases; while both find correspondence in Taxology, or the relationship of organic forms now existing on our globe. The correspondence, one with the other, of these three great series is one of the most pregnant facts of biology and inexplicable on any other theory than that of derivation. It is the essence of evolution that the present more complex forms are derived from older and simpler forms and that the greater the difference between groups the greater the divergence and the deeper down the converging stem or ancestry. We are justified in concluding that complete knowledge (were that possible) would permit us to trace back the origin of every organism to the simple unicellular, ancestral form. I would hardly second the strictures of a former vice president of this section on the "scientific arboriculturists," because philosophy must needs lead from the known to the unknown, and the value of a phylogenetic table depends on the knowledge and ability displayed in projecting it and on the plausibility of those parts which are not based on palæontological facts—and perhaps never can be—but which the facts of embryology suggest. Biologic evolution implies, further, what has hardly been sufficiently emphasized, (1) increase in size or bulk as correlated with the increasing diversity and complexity of organization; (2) that the most highly organized require (comparatively) the longest time for development both in the fœtal and postnatal states, and (3) that large organisms with complex structure are more sensitive to changes of condition and more liable to extinction; and this means that the larger the size and the higher the development, the fewer in number both as to species and specimens.

These principles, now so generally accepted, aside, let us at once throw a *coup d'œil* over some of the factors of evolution and see what we understand of the causes of variation and differentiation, as this is the question of questions among biologists to-day.

The essence of Darwinism as an element in evolution, viz., "natural selection," was original with several, and Darwin himself remarks, in his autobiography, that an essay in Hebrew had been published showing that the theory is contained in the Old

Testament. He also recognized that Bronn in his "Geschichte der Natur" forestalled him in many ways. Even as early as 1766 Duchèsne wrote profoundly of the lines of variation and evolution, and showed that a genealogical order is the only one which satisfies the mind, all others being arbitrary; and he ventured a genealogical tree based upon profound knowledge and ascertained facts in the natural history and cultivation of the strawberry. The principle was also fully recognized in 1813 by Dr. W. C. Wells of Charleston, S. C., and in 1831 by Patrick Matthews, as later editions of the "Origin of Species" set forth. Such, however, is the history of all great theories. They cannot be cast at once full-panoplied and impregnable like Minerva from Jove's head. Great discovery is usually more or less definitely foreshadowed.

The publication of the "Origin of Species," however, marked a new epoch in biology and the work has profoundly influenced modern thought. Yet Darwin's fame rests just as firmly upon that vast structure of facts which he so successfully brought together in his various writings, as it does on his theories; and had he never propounded the theory of Natural Selection, his writings would have immortalized him, for they form an encyclopædia of well-arranged data for the naturalist, the agriculturist, and the stock-breeder. He threw the light of his genius into recesses formerly obscured and opened new vistas through old problems which had previously defied elucidation.

It would require volumes even to indicate the extent and character of the literature upon evolution since the appearance of the "Origin." The proceedings of this section of the American Association during a quarter of a century have been, in a measure, typical of the proceedings of like bodies the world over, and constitute a record of the discussions and of the progress of thought and experience in this direction. We have every reason to be proud of the work of American biologists as illustrated in this record; for, notwithstanding the vagaries of a Swallow, and the more able and serious opposition of a Dawson, belief in the derivative origin of species has steadily gained among biologists and now includes all those whose work and word are worthy of consideration. We may be proud, also, of the demonstrative proof which members of this section have brought to bear upon the general theory, as also of the newer thought and far-reaching generalization original with other members of the section.

In looking over this record two things strike me as worthy of further consideration. On the one side there has been a disposition to widen the meaning of the term "Natural Selection" so as to include the cause, or causes, as well as the method, of variation and modification. On the other side, the tendency has been to the opposite extreme or to limit the application of the term to the mere selection of the fittest, so that it becomes but the expression of a common and easily observed fact in nature, without involving any of the more fundamental principles of evolution. It becomes merely a means or method and in no sense a cause of modification. Some writers even go so far as to insist that while it is all powerful in originating genera, *i. e.*, in producing adaptive structure, it has little or nothing to do with the production or origination of species and they would thus render the very title of Darwin's greatest work a misnomer.

It may not prove unprofitable to note what the "Life and Letters" have to say upon this question, and to see what limit, if any, Darwin himself placed upon the term. It is of prime importance that we use it in as accurate a sense as possible, since it has played such an important part in the literature of, and expresses such an important factor in, evolution. The book is most interesting and suggestive; for we not only get from it an insight into the persistent and laborious effort which resulted in Darwin's enduring fame, and left so deep an impression on the scientific work of his generation; but we come to realize how he labored in giving his thought that forceful, logical, yet simple expression which had so much to do in making his work popular; how the candor of his argument and statement was but the reflex of the candor and honesty of his mind. We learn to appreciate more fully the vast range of knowledge he possessed of scientific fact, both from personal experience and authority, and how he bent it to one great end. We come to love him for his many beautiful personal traits; his noble character; his simplicity; the courage with which he bore up under bodily ailment; and for his humanity to animals, which is well brought out by interesting anecdotes given by his son. We honor him for his strong feeling and sympathy with suffering, both in man and beast; for his horror at the suffering of slaves, which inclined him strongly to the Union side in our Rebellion but did not blind him to what, from the average English standpoint, were the political issues at stake; just as his sensitiveness to the suffering of

animals did not warp his position on the question of vivisection, which he believed to be justified for investigations in physiology. In scanning the pages of his personal record we come to realize fully that

> "His life was gentle; and the elements
> So mixed in him that nature might stand up
> And say to all the world—
> This is a man!"

But aside from the insight which the book gives into the lovable character of the man, and of his method of work, it is replete with thought and fact, and may be looked upon almost as an appendix to the "Origin." Let us see, therefore, what light it throws on the question we have propounded.

The actual causes of variation may be few or many—remote or immediate; but their discovery or non-discovery no more affects the principle of natural selection than the difficulty in elucidating the causes of gravitation affects it as one of the grandest discoveries and generalizations of our age. We may come to understand, and are already able to elucidate some of the proximate causes, but the consideration thereof inevitably leads us back farther and farther to the great First Cause, and Darwin's work would never have had so profound an influence had he, instead of basing his theories on demonstrable and experimentive fact, been led into the more speculative realms of causation. Not but that he was intensely interested in the causes of variation; for we have his own words and those of his son to show the ever-present desire in his mind to learn something thereof. But he avoided consideration of them in the same way that he avoided speculation on the origin of life itself, seeing clearly, no doubt, that both questions lead ultimately to Infinite Causation and that this is beyond man's finite comprehension, in his present state of development, at least.

In a letter to Lyell, September 12, 1860, in response to a question why rodents have not become more highly developed in Australia, he says: "I feel that our ignorance is so profound, why one form is preserved with nearly the same structure, or advances in organization or even retrogrades, or becomes extinct, that I cannot put very great weight on the difficulty." Again, Feb. 23, 1860, he says: "With respect to Bronn's objection that it cannot be shown how life arises, and likewise to a certain extent Asa Gray's remark that natural selection is not a *vera causa*, I was much interested by

finding accidentally in Brewster's 'Life of Newton' that Leibnitz objected to the law of gravity because Newton could not tell what gravity itself is.* * * Newton answers by saying that it is philosophical to make out the movements of a clock, though you do not know why the weight descends to the ground."

We may now consider the question of natural selection itself, and see whether, as the expression of a principle, it may not be more strictly defined than it was by Darwin himself.

We find in his correspondence with Wallace that the latter states that the term "survival of the fittest" is the plain expression of a fact. Natural selection, on the contrary, is a metaphorical expression of it, but to a certain degree indirect and incorrect. The great objection which Darwin urged to the term "survival of the fittest" was that it could not be used as a substantive governing a verb, which obliged Spencer himself continually to use the term "natural selection." Darwin recognized, however, the force of the objections to the term, and yet it would be difficult to find a better, and his final preference, after long deliberation and correspondence with men like J. D. Hooker, has justified his judgment. Wallace noticed that Darwin had used the term in two senses: *first*, for the simple preservation of favorable and destruction of unfavorable variations, in which case, according to Wallace, it is equivalent to the "survival of the fittest," and *secondly*, for the effect of the change produced by this preservation. In his autobiography, Darwin says: "But it was clearly evident that neither the action of the surrounding conditions nor the will of the organism (especially in the case of plants) could account for the cases in which organisms of every kind are beautifully adapted to their habits of life." Nothing can be more characteristic than the following in his long letter to Lyell, October 11, 1857. "It has taken me so many years to disabuse my mind of the too great importance of climate —its important influence being so conspicuous, while that of the struggle between creature and creature is so hidden—that I am inclined to swear at the North Pole and, as Sidney Smith said, even to speak disrespectfully of the Equator." In a letter to Victor Carus we find him inclined to place more value on the definite action of external conditions, and to infer that single variations are of less importance in comparison *with individual* differences than he formerly thought.

I recollect well, while visiting him in the fall of 1871, that he

expressed very much the same views which he has expressed in a letter written about that time to Huxley, in which, using the illustration of a pendulum, he says: "The pendulum is now swinging towards our side and I feel positive that it will soon swing the other way." He realized that there would be oscillations in the popularity and general acceptançe of his views and especially as to the part of natural selection as an originating power.

Thus his own views as to the value, scope and bearing of natural selection, varied to some extent, and he used the term in two different senses. In the broader sense, as used by him, and by many of his followers, notably in this country by Fiske, Morse, Marsh, etc., it is a great principle of modification that includes both the fact of variation from whatever cause or causes and the explanation of accumulative divergence along beneficial and adaptive lines. It involves the Malthusian struggle for existence, not only among the organisms themselves but with the elements and the environment.

It is plain from Darwin's own writings that a term to express the principle was not easily found and the difficulty was doubtless due to the uncertainty that existed in the author's mind, as it has existed in the minds of his followers, as to the exact limitations of the principle. The term is happy in my judgment, because it has permitted the focussing of definition by subsequent elucidation. Darwin could but feel that "Wahl der Lebensweise," as a German translation, hardly expressed his opinion, and he was right; "natürliche Zuchtwahl," the later translation adopted, being far preferable.

As propounded by him, natural selection deals essentially with the variation of the individual under like conditions, as distinguished from the variation of the type under change of environment. He impersonates by the term an *ensemble*, *i. e.*, a number of innate conditions of variation. The principle is, in fact, based upon the Leibnitzian axiom "*Natura non agit saltatim*," and it finds a counterpart in the facts of artificial selection induced by man which, in reality, led Darwin to adopt the term to express selection by nature. But here it must distinctly be borne in mind that by Nature, though it is difficult to avoid personifying it, he meant "only the aggregate action and product of many natural laws,— and by laws only the ascertained sequence of events. He found it difficult to admit of any personality or designer in any way using natural

selection for designed ends, so that the comparison with artificial selection by man lacks in one most important particular; but we may recur to this point later on. Natural selection is a great principle, the promulgation of which revolutionized biology. It is a *modus operandi* of derivative genesis, embodying two somewhat opposing laws, namely, heredity and adaptation; the former for the most part conservative and tending to cause organisms to hold to the past, the latter progressive and tending to cause them to diverge or ramify. It is a *quo modo* of succession, but it implies no necessary tendency to progression, however much such progress may be indicated in the general history of evolution. Certain simple conditions of life may have persisted from before the Silurian age to the present day, and the one primordial prototype of all living and extinct creatures may still exist so far as natural selection is concerned.

It is a principle universal in its action, explaining one important mode of modification and differentiation of forms, especially among higher animals or where the interests of highly endowed or organized beings most strongly interact to give it effect. It must be less effective among lower organisms, where external conditions are evidently prepotent in inducing not only variation but specific modification. Natural selection, therefore, does not satisfy us as an explanation of the original differentiation of the great classes, and it was perhaps a certain recognition of this fact which caused Darwin to pause on the safe side of their differentiation and not endeavor to explore the deeper mysteries beyond. Yet after all it is among these lowest forms that the key to the explanation of the more important factors in evolution must be sought.

The Darwinian, therefore, who would give fullest expression to the teachings of the master, employs the term in a broad sense as the expression of a principle in nature which explains how the fittest have come to be preserved, and hence the mode of formation of species. The "formation" would have been a happier expression than the "origin" of species, but not so good a catch-title, and we should never forget that the sub-title of the immortal work, viz., "The preservation of favored races in the struggle for life," more fully expresses the author's meaning. By its too zealous advocates natural selection has been used to explain phenomena due to other causes. This is, however, a danger that all great discoveries encounter; they are made to do service for which they were not originally propounded. But the opposite tendency, when car-

ried to extreme, is equally unjustified and would obliterate natural selection as a factor in evolution.

Spencer's expression the "survival of the fittest" has often been used as synonymous with natural selection, to the detriment of the latter; for while all must recognize the aptness of the term, as a somewhat tautological expression of the result, it can never be made to cover the principle involved in the Darwinian term. It is an explicit and forceful expression of the fact and in no sense of the theory. Natural selection deals not so much with the survival of the fittest as with the destruction of the unfit. It expresses a cause of differentiation or formation of species and higher groups, in a sense in which the "*survival* of the fittest" does not, however little either term may explain the causes of variation *per se* or the *origin* of the individual variation. It operates under subjection, taking advantage of variation otherwise initiated; or, to use Spencer's language, it expresses an effect of the mode of coöperation among causes. We are justified, therefore, in saying that natural selection is the essence of Darwinism and is a fundamental principle in organic evolution based on (1) individual variation and (2) the struggle for existence and preservation of the most competent. Other factors of less importance help to give it potency. It is based upon the facts as they may be observed, and the whole superstructure is built upon the innate variability of individuals, irrespective of conditions. The difficulty of getting at the immediate cause or causes of this individual variation led Darwin to consider it promiscuous or aimless, though he wisely avoids calling it lawless. He felt, as we have already seen, that there were causes and that of the majority of these we were ignorant. In his own words, "we can so rarely trace the precise relation between cause and effect, that we are tempted to speak of variations as if they arose spontaneously. We may even call them accidental, but this must be only in the sense in which we may say that the fragment of rock dropped from a height owes its shape to accident."

I have always had a feeling, and it has grown on me with increasing experience, that the weak features of Darwinism and hence of natural selection, are his insistence (1) on the necessity of slight modification; (2) on the length of time required for the accumulation of modification, and (3) on the absolute utility of the modified structure. I think that Darwin laid altogether too much stress on these points, and that while, in the main, insistence thereon

is justified, a too strict adherence to them weakens natural selection as a true expression of the phenomena of modification. This is particularly true of lower organisms among which, as we have already seen, natural selection has been and is necessarily less potent than among the more highly organized and complex, from which, especially under domestication, Darwin drew most of his evidence.

Whatever influence we may attach to environment and external conditions, it is self-evident that they alone have not been sufficient to induce the wonderful variety of life existing upon the globe to-day. Indeed, so far as natural selection implies necessary utility, necessary adaptation to surroundings, it is, as I have said, defective. We know very well that introduced species from one continent to another, or from one country to another, have proved better adapted to the changed conditions than the indigenes or endemic forms. This is readily comprehended on two grounds; *first*, that species which have, in the course of time, experienced a greater struggle among themselves in large areas, have an advantage over those in more limited areas in which the struggle has been less intense; *secondly*, that species which have accommodated themselves to the changes in life conditions which civilized man induces, have a great advantage when, following man's migrations, they are brought into competition with species which have not yet been subjected to such conditions. Again, no valid reason can be urged why, within a given area, one species predominates over another in so far as mere adaptation is concerned. The influences of environment alone would tend to unify the fauna and flora of a given region. Theoretically, so far as climate and physical conditions are concerned, there is no reason, through regions where these are uniform, why a single animal should not prevail to the exclusion of all others, providing it was vegetarian, or that the particular plant which furnished food to such an animal should not prevail to the exclusion of all others. The hickory and the blade of grass must be considered equally adapted to the environment with the oak, and so on all through the multifarious forms of both vegetal and animal life: so that this diversity of form can best be explained by some principle like natural selection, and by the interrelation and interaction of organisms and the struggle between them for existence. This is illustrated in many directions. To take a striking example: no one doubts that if the larger carnivora

of Europe and Asia were introduced into Australia, the marsupials would soon have to give way and could survive only by the acquisition of special functional modifications and larger intelligence such as we find in our opossum. Yet it would be folly to conclude that the marsupials are less well fitted to the physical conditions which obtain in Australia than their introduced exterminators.

From what has preceded, we are, I think, justified in rejecting the interpretations of both extremists as to the scope and meaning of natural selection. It cannot be debased to the mere expression of the universally observed fact of variability; yet it must be restricted, because it not only implies something to be selected, but its promulgator limits its scope to the selection of something that is useful. As a philosophy it considers only processes and leaves remote origin and cause untouched. The following limitations are probably justified to-day and will help to more exact use of the term.

1. It deals only with individual variation from whatever cause, and should not be applied to simultaneous variation in masses.

2. It deals only with variations useful to the organism in its struggle for existence, and can exert no power in fixing the endless number of what, from present knowledge, we are obliged to consider fortuitous characters. It cannot perpetuate useless organs; nor those of a vestigiary or obsolescent character.[1]

Even with these restrictions, the principle is far-reaching and profoundly important; but it quite fails to account for many of the most interesting manifestations of life that are obviously not necessary or life-preserving, of which many will occur to every one, such as, among lower organisms, many superficial details of structure; or, as among higher organisms, odd habits and customs, playful instincts, ethical traits, etc. Its limitations must be narrowed in proportion as we come to understand the other laws of modification and the causes of variation in masses. Let us briefly consider some of these causes.

While, as already stated, the consideration of this question inevitably leads to Ultimate Cause, there is no more fascinating or profitable field of investigation than that leading to the proximate cause or causes of variation. We are not content to rest the case where Darwin did by recognizing variation as an inherent principle

[1] In the literature of evolution, these are usually termed rudimentary, but, strictly speaking, this term should be applied only to nascent or incipient structures.

in organic forms, or to beg the question by saying that it is as much a necessity of life as natural selection itself. Let us, therefore, discuss these causes in the light of recent experience and experiment.

We soon find that they admit of a certain amount of classification, the minor divisions of which, as in all systems of classification, more or less fully interlock or blend. They fall, however, into two chief categories, viz. (1) external conditions or environment, which are, at bottom, physical, and (2) internal tendencies or promptings, which are, at bottom, psychical. We shall also realize more fully that there is good reason for the varying importance which has been placed on natural selection because it represents a broad principle, based on the outcome of both these categories, but particularly of the latter. Its value is not a fixed one, and must needs change with the increase of exact knowledge of the other factors, and did in fact change in the mind of its originator. We shall further find that there are laws of evolution which permit of formulation and expression, and which have influenced or controlled the mode of variation, but which must not be confounded with or included among the causes of the variation proper, though here again, the line between the two kinds of factors is not always easily defined.

The conditions of organic modification may, therefore, roughly be classed as (A) external and (B) internal, and these may be almost indefinitely subdivided. The former class includes (1) *physical* and (2) *chemical* forces and in a broad way may be said to induce modification independently of natural selection, however much this may act with them as a secondary cause. Certain prominent features of the physical forces are worthy of mention: as light, temperature, water (stagnant, or in motion), climate (under which term may be included meteorologic phenomena, as electricity, atmospheric pressure, etc.), mechanics (gravitation, wind, stress, friction, etc.) and geographics (migration, isolation, etc.). The chemical forces may be considered under the subdivisions, aquatic, atmospheric, food and soil. In class A may also be included (3) *vital*[1] or organic force in so far as this is concerned with the in-

[1] I am well aware that this term is much tabooed among a certain class of the more materialistic evolutionists, but I use it here for want of a better, and because as an expression of one form of manifestation of force, it has as much a classificatory value as physical or psychical.

teraction of organisms, and it is seen thus to link the two great classes. The second class (B) includes (1) *physiological* and (2) *psychical* forces. Prominent among the former, as causes of modification, are worthy of mention those connected with genesis itself: as heredity, physiological selection, sexual selection, hybridity, primogenital selection, and what I would call sexual differentiation, and philoprogeneity. Among the latter may be included use and disuse, individual effort, etc.; and last, but not least, the emotions.

As already stated, any such classification of the forces at work in organic evolution must be more or less arbitrary and artificial. Fundamentally also, they are, perhaps, convertible terms—unifiable—one. But some such arrangement as that here suggested serves to simplify discussion.

Now with the limited definition given to natural selection, all the forces in class A act independently of it, while the rest are more or less fully aids to its action. Time will not permit of much detailed consideration of the physical and chemical forces. Nor is such consideration necessary; for their influence, as Darwin well remarked, is obvious. Fundamentally, they must needs limit and control all manifestations of life of which indeed, on evolutional grounds, they are the material basis. Change of physical environment may affect function first and chiefly, but this involves change of form and structure which are integrated by heredity. The surface of the earth and the waters upon it and the atmosphere above it have necessarily conditioned the chief modes of animal locomotion as swimming, flying, crawling and walking, while the five great classes of vertebrates find the explanation of their structure, as J. B. Steere pointed out at the Ann Arbor meeting, in the conditions of life in water, in shallows, in the air, on land and on trees and rocks.

External Conditions.—By external conditions or environment, we include all influences on organisms which act from without, and in carefully considering them we shall find it difficult to draw the line between those which are really external and independent of any motive or inherent tendency in the organism, and those which are not. Hence, the general term "External Conditions" is resolvable into various minor factors. Considering the influences as a whole, we find that in the 1844 essay, or sketch, Darwin gave more weight to them as producing variations, and as modifying habit, than he

did in the "Origin"; yet we all know that he felt convinced when this work was first issued, that natural selection was the main, though not the exclusive, means of modification. Before his death, he was again led to attach greater importance to them. As late as March, 1877, he wrote to Neumayr, of Vienna, that "there cannot be any doubt that species can be modified through the direct action of the environment. I have some cause for not having more strongly insisted on this head in my 'Origin of Species,' as most of the best facts have been observed since its publication." He was led to this modification of his views by Neumayr's essay on "Die Congerien," and by Hyatt's work in showing that similar forms may be derived from distinct lines of descent. In his correspondence with Huxley, Darwin remarks that one point has greatly troubled him. If, as he believed, accidental conditions produced little direct effect, "What the Devil determined each particular variation? What makes the tuft of feathers come on the cock's head, or moss on the moss rose?"

It is quite plain, indeed, that subsequent to the publication of the "Origin," and especially in 1862, in his correspondence with Lyell, Darwin was inclined to give more power to physical conditions, and, in fact, was wavering in his mind as to the force of the different influences at work. In his letters to Hooker in 1862, the same tendency may be noted and the preparation of the "Variation of animals and plants, under Domestication," led him to believe rather more in the direct action of physical conditions, though he seemed to regret it because it lessened the glory of natural selection and, to use his own language, "is so confoundedly doubtful." One can plainly trace from the correspondence how, prior to the publication of the "Origin," he more and more, as his facts accumulated, and as the theory of natural selection grew upon him, relegated to an inferior place the influence of environment; while, subsequent to the publication of that work, and up to the time of his death, the tendency seemed to be in the opposite direction.

Many eminent workers have differed greatly from Darwin in the influence allowed to these external conditions, and this is particularly the case with our American writers. Indeed, no one can well study organic life, especially in its lower manifestations, without being impressed with the great power of the environment. Joseph LeConte speaks of the organic kingdom lying, as it were, "passive and plastic in the moulding hands of the environment." Leidy,

Wyman, Clark, Packard, etc., have insisted on the influence of physical conditions. Baird and Ridgway on geographical distribution, Whitman on concrescence, Hyatt on gravitation, Cope and Ryder on mechanical stress, have all published valuable corroborative evidence; while many other writers have added their views and testimony which have been admirably condensed by Professor Morse in two addresses before this Association. Allen demonstrates plainly the influence of climate and temperature in directly inducing specific changes. Weismann, in his remarkable "Studien der Descendenz Theorie," concludes that differences of specific value can originate only through the direct action of external conditions, and that allied species and genera, and even entire families, are modified in the same direction by similar external inducing causes. In Semper's "Animal Life" (1877) we have the best systematized effort to bring together the direct causes of variation, and no one who has read through its pages can doubt the direct modifying influences of nutrition, light, temperature, water at rest and in motion, atmosphere still or in motion, etc., or question his conclusion that no power which is able to act only as a selective and not as a transforming influence, can ever be exclusively put forth as a *causa efficiens* of the phenomena. Kölliker, in 1872, wrote: "Manifold external conditions, when they operate on eggs undergoing their normal development, on larvæ or other early stages of animals, and on the adult forms, have produced in them partly progressive, and partly regressive, transformations," and recognized as most important forces, nutrition, light and heat. Indeed, the direct action of environment must have been, as Spencer puts it, "the primordial factor of organic evolution."

In so far as it offers evidence, entomology confirms the conclusions of the writers in other departments of natural history, above referred to, and offers a host of most conclusive proofs of the direct action of the physical and chemical factors which I have enumerated. Justice, however, could not be done to the facts within the limits of an address of this kind, and I pass on to some of the other factors.

It is among what I have called the vital or organic conditions of variation that natural selection has fullest sway, and as they have been so ably expounded by Darwin and others they may be dealt with in few words.

Interaction of Organisms.—The productions, as a whole, of greater

areas will, whenever they get an opportunity, conquer those of lesser areas, and in this broad sense, the interaction of organisms may be said to have had no special modifying power, however great its influence may have been, and is yet, in inducing the survival of the fittest, or in bringing about the present geographical distribution of species. The consequence of enforced migration and of isolation are best considered when dealing with the physical conditions, because they must influence modification of masses rather than of individuals, and either substitute one type for another or remove competing or differentiating influences. But in the more restricted sense, *i. e.*, the interaction of organisms occupying the same ground—the struggle for existence, in other words, between direct competing organisms—is a prime Darwinian factor of modification, and a whole volume of illustrations may be drawn from entomology; for in no class is the contest more severe, whether with plants, or with other animals, or with one another, than in insects. In no other field of biology, for instance, have the physical conditions resulted in such infinite diversity of form and habit fitted, whether for earth, air or water, and often for all in the same individual; so, also, in no other field, is parasitism carried to such a degree, or are the purely adaptive structures due to this interaction so varied or so remarkable. The entomologist who goes beyond the "dry bones" of his science is inevitably a Darwinian.

In this category must also be included that interrelation between insects and plants which has eventuated in the so-called carnivorous plants, and that still more wonderful interaction between flowers and insects by which each has modified the other, and the facts of which have been so untiringly observed and so well set forth by a number of writers from Sprengel's day to this, and by none more successfully than by Darwin himself. These are plainly inexplicable on external conditions acting on masses alike and are meaningless enigmas except on the theory of natural selection, or some supra-natural and dogmatic gospel.

We are thus led, through this last, from the external to the internal factors in evolution, or those of a physiological and psychical nature. In these, natural selection is the key which, so far, best unlocks their meaning and shows how they have acted in the formation of species and the less fundamental of the great groups. In considering them it is hardly necessary to discuss their relative importance as compared with the external conditions, though it

may be remarked that they are the factors which have induced the great variety of adaptive forms and minor differentiations, while the external conditions have governed the formation of the great and more comprehensive types of structure.

Darwin was led to give more importance toward the end than he had originally done, to some of these internal factors and especially to functionally-produced modifications. In the "Descent of Man" he says that he did not sufficiently consider variations "which so far as we can at present judge are neither of benefit nor injurious; and this I believe to be one of the greatest oversights I have yet detected in my work." And in the sixth edition of the "Origin" he frankly admits that he had omitted in other editions to consider properly the frequency and importance of modifications due to spontaneous variability. He further refers to morphologic differences, which may have become constant through the nature of the organism and the surrounding conditions rather than through natural selection, since they do not affect the welfare of the species. In short, Darwin's views kept pace with the investigations of his day and tended in the direction of restricting rather than widening the influence of natural selection. But, as Romanes, and especially Spencer, in his Factors of Evolution, have fully shown Darwin's position on this subject, I may pass over the detail.

INTERNAL CONDITIONS—*Physiological.*—*Genesis* itself is the first and most fundamental of all causes of variation. The philosophy of sex may, indeed, be sought in this differentiation, as the accumulated qualities in separate entities when suddenly conjoined or commingled inevitably lead to aggregation and heterogeneity—in other words, to plasticity or capacity to vary. Genesis, as a fundamental factor in evolution, may be more intelligently considered under some of its subordinate phases, as heredity, physiological selection, sexual selection, primogenital selection, sexual differentiation including philoprogeneity, hybridity, etc.

Heredity, as expounded by the ablest biologists and as exemplified in life, is a puissant factor in evolution and though essentially conservative must, through the marvellous power of atavism, tend to increase individual variability. The subject has been too well considered by Darwin and his followers to justify further discussion of it here. As a cause of variation, heredity must, however, have less and less influence as we go back in the scale of organized beings; for it cannot well come into play in agamic or fissiparous re-

production, a fact which has given the abiogenesists one of their strongest arguments, since it is difficult to understand how, for instance, the monera of to-day could have descended without change from the primordial form.

Physiological Selection.—Physiological selection, as suggested by Mr. Catchpool and as expounded by Romanes, is undoubtedly a veritable factor in evolution, and while giving us another link in the chain of evidence as to the causes of differentiation, lessens in but very slight degree, the overwhelming force of the argument for natural selection. It adds, rather, an important element in the evidence therefor and may be classed as a subordinate cause of differentiation. Romanes' theory is based upon the argument that differences, such as constitute varieties and species in their commencement, would not be preserved by natural selection unless useful, but would be lost again by cross breeding with forms like the parent, and which had not varied, except upon some hypothesis like that of physiological selection. This could not be prevented except by migration. This difficulty is a general one, was argued by Darwin himself, and has been felt by all Darwinians. The reproductive organs are extremely variable and sterility may occur not only between species, but between races and varieties and often between individuals. Physiological selection tends to form varieties by peculiarities in the reproductive system of individuals which render them unfit for perfect coition, or cause them to remain more or less sterile, with other individuals which have not the same peculiarities.

The exact reasons are recondite, and the whole subject difficult of demonstration except from the results, since changes in the reproductive organs are not easily observable. Romanes believes this sterility to be incidental to variation and hence one of the chief causes of the accumulation of such variation. Wherever there has been modification of the reproductive organs introducing incompatibility between two individuals, even where there has been no other change or variation, we have a valid cause of differentiation which in its consequences must be important. Compatibility or fertility between individuals is of the very essence of selection. Natural selection implies that this sexual divergence is subsequent to or coincident with divergences in other directions; physiological selection, that it antecedes them. To put the case of Romanes more fully, we will suppose that among the natural variations there occasionally

occurs something to affect the reproductive organs in such wise as to produce incompatibility, *i. e.*, incapacity of one individual with another of the parent type, to unite, or sterility of such union, while it remains fertile with the variation of its own kind. This theory of course implies variation in the reproductive organs, or departure from the parental type, in at least two individuals of opposite sex simultaneously, and with this admission, for which we are justified in facts, physiological selection will preserve many peculiarities which need have no necessary connection with the exigencies of life.

The change may be in the organs of reproduction, introducing sexual incompatibility, or it may be due to other causes, as, for instance, the time of flowering in plants, or the season of heat in animals. Even the element of scent becomes important here, as my friend J. Jenner Weir has suggested, since it may influence sexual relationship, so that the very excretions of the body, which vary with individuals, must be allowed their part. Francis Galton has indicated a modification of Romanes' views, viz., that the primary characteristic of a variety resides in the fact that the individuals who compose it do not care to mate with those outside their pale. Incipient varieties are thus thrown off from the parent stock by means of peculiarities of sexual instinct which prompt what anthropologists call endogamy and check exogamy or marriage without the tribe or cast. This is a very good anthropological illustration of how physiological selection may begin.

Natural selection preserves the individuals best adapted to life conditions by destroying the less fit. Physiological selection may be said to preserve differences which have no necessary connection with the necessities of life. Neither touches the origin of the variation, but both express laws thereof or methods by which it is accumulated. The inherent tendency to vary, whether in external or adaptive structure, or internal or reproductive character, is simply an observed fact, the causes of which we are endeavoring to analyze.

Physiological selection is remarkably exemplified in insects and probably in no other class are the modifications which may be attributed to it more easily studied; for in no other class are the genitalia of the male so variable or so complex. There has so far been no attempt to homologize the different parts in the different orders of insects, so that they have received different names ac-

cording to individual authors. Ordinarily there are two pairs of claspers, themselves very variable, associated with sundry hooks and tufts of hair. There are families, as in the Cecidomyidæ, among the Diptera, in which many species are almost, and others, absolutely, indistinguishable except by the differences in the male genitalia. In all other orders there are an immense number of forms which can only be distinguished by a careful study of those organs. Descriptive entomology to-day, which does not take account of these organs, is, in fact, almost valueless, and we must necessarily assume that where there is differentiation of structure in these important parts it implies a corresponding modification on the part of some associated female even where no other differentiated characters are to be detected, and upon Romanes' law such must be looked upon as physiological varieties and will be counted good species in proportion as the differentiation involves other observable characters or as their life habits determine.

Sexual Selection. — The part of sexual selection in inducing variation may next be considered. While it is evidently at the bottom of the diversity in sex so common among many animals, it is difficult to see how it can play any very important part in the differentiation of species, except on the hypothesis that the greater the differentiation between the sexes the greater the tendency to vary in the offspring. In no class of organisms is this factor more notable than in insects, and volumes might be written to record the interesting and curious facts in this class alone. As a general rule it may be said that with insects, as with other animals, it acts chiefly in inducing secondary sexual characteristics in the male, and in simplifying the characteristics of the female. Nowhere do we find greater contrasts between the sexes, involving almost every organ, both colorationally and structurally. Where color is affected, the greater brilliancy almost always belongs to the male sex, as in birds. So where song or sound is employed to attract, the sound organs are either peculiar to, or most highly developed in, the males. As in higher animals, also, so in insects, we find offensive organs highly developed in the male, and either lacking, or but partially developed in the female, wherever the struggle for the possession of the female is by force, or strength. It has evolved scent organs in various parts of the body, causing modification, especially in the Lepidoptera, of either the membrane of the wing, or the scaly covering; it has induced profound modification in the

structure of the legs, whether the anterior, middle or posterior pair, and whether in the whole member or some part of it, or in its covering. The subject has been so fully treated by Darwin, however, that it is not necessary to elaborate it further in this connection. Strictly speaking, it may be said to act in two ways, viz.: by conflict of the males for possession of the female, or by attractiveness, the former being most conspicuous among mammals, the latter among birds, and both coming conspicuously into play among insects. It is rather difficult to define the limit of sexual selection as a factor in evolution, but I would not confound it with another factor, not hitherto generally recognized, but which I think must be all-powerful, namely, sexual differentiation.

Sexual Differentiation.—It seems evident that the mere differentiation of sex in itself has been an important element in variation. The principle elaborated by Brooks as a modification of the theory of pangenesis is a good one, and in the main the male may be said to be the more complex and to represent the progressive, and the female the more simple and to represent the conservative element in nature. When the conditions of life are favorable, the female preponderates, and exercises a conservative influence. When the conditions are unfavorable the males preponderate and with their greater tendency to vary induce greater plasticity in the species, and hence greater power of adaptation. Sexual differentiation may, I think, be used to include many other variations and differentiations not otherwise satisfactorily accounted for, and to express the law of the interaction of the sexes upon one another, inducing great differentiation entirely apart from the struggle of the males for the possession of the females, or the struggle for existence. Among insects, particularly, though the same is true among other classes, we find many illustrations of this that can hardly be explained by the other forms of selection.

A few of the more notable in Hexapods may be instanced, as the degraded form of the female in Stylopidæ; in very many Lepidoptera and Coleoptera; in the females of the Coccidæ, in Homoptera, etc. In most of these cases it is the female which has been modified, without any very special modification in the male, though it is a general rule that in proportion as the female is degradational and stationary, the organs which permit him to find her, or to mate with her, and particularly the antennæ, eyes and genitalia are profoundly modified and complex. This is especially noticeable in

the Psychidæ where the female remains in her case, a mere mouthless, eyeless, legless and wingless grub, and the male has most complex and ramose antennæ and complex genitalia. Another remarkable instance may be cited in the Lampyridæ, where we find every degree of degradation in the female, from partial wings to no wings at all, accompanied with increasing complexity of eyes and antennæ in the male, until at last in the Phengodini the female is so larviform that she can hardly be distinguished from the true larva. In all these cases the female has been as profoundly modified as, and often more so than, the male, and in the latter case a phosphorescent power has been evolved so that the attractiveness, as in the human species, is rather on the female side. Again, in the case of Corydalus, in Neuroptera, the profound modification of the jaws in the male into prehensile sickle-shaped organs is to be explained rather on the interaction between the sexes, and the facility the modification offers for coition, than upon sexual selection in its proper and restricted sense.

In this category must also be included the influence of *philoprogeneity* which has modified the female rather than the male either in the primary sexual organs for offence or defence, as in the sting of the aculeate Hymenoptera; or in the secondary sexual characters, as in the anal tufts of hair, secretory glands, etc., of many Lepidoptera; or in modification of various other parts of the body exhibited in various orders of insects to facilitate provision for their young, whether in the preservation of the eggs or the accumulation of food for the future progeny. A notable instance of how far this may be carried is furnished by the female Pronuba, where the ovipositor and the maxillæ are so profoundly modified as to make her unique in her order. Sexual selection can have little to do with these modifications, cases of which might be multiplied indefinitely; nor can they be fully explained by natural selection, in the restricted sense in which we have proposed to use it; nor by physiological selection.

In this category might also be included modification which has resulted in the various forms of females which obtain in the same species, fitted whether for agamic or sexual reproduction and which are far more readily explained on the theory of sexual differentiation aided by environmental influence, especially food and temperature, than upon any other.

Hybridity.—The subject of hybridity has been fully discussed by many and by no one more ably than by Darwin himself. It

has generally been assumed that the hybrid of any two species is sterile, and, in fact, hybridity has been looked upon as one of the best tests of specific value next to genesic incapacity. The assumption finds its greatest support in genesis among the higher animals, and the most thoroughly differentiated species; but the whole subject becomes complicated as we descend in the organic scale, and hybrids between what naturalists generally separate as good species are far more frequently fertile among plants and lower animals than was formerly supposed; while physiological selection, as we have just seen, may render genesis impossible, or at least prevent it, between varieties and incipient species. In this light, hybridity becomes an important factor in the modification of species. Unnecessary importance has been given, in my judgment, to the fact that domestic and wild species differ in the fertility of their crosses. It is assumed, for instance, that all the known breeds of domestic dogs would be fertile *inter se* and produce fertile crosses. It seems to me, on the very face, a preposterous proposition and that many of the breeds of domestic dogs are as distinct specifically, and even generically, so far as this test is concerned, as they are in structure and other characteristics. Who, for instance, has ever known or heard of a cross between a bull dog and a lap dog, or between a Newfoundland and a black and tan? The difference in size alone would seem to render such a cross, if not a physiological or a physical, at least a practical, impossibility; so that hybridity among domestic animals tends to essentially the same result as among wild animals, and confirms its importance as a differentiating factor.

Having thus summarily indicated those factors of evolution associated with genesis and which are essentially physiological, however much psychical phenomena may coöperate, we may touch upon the more purely psychical factors or those pertaining to the growth and use of mind, employing the term to express those neural phenomena traceable to the medium of the brain. Their importance in evolution increases with increasing cephalization and complexity of nerve system. For the present purpose, however, it is with the objective side of psychology or what may be called psycho-physiology that we must deal.

Psychical—Use and Disuse.—Full consideration of the effect of use and disuse involves a discussion, not only of the question of the transmission of acquired structures, but of the influence of individual effort and of necessity, *i. e.*, a consideration of the essen-

tially Lamarckian factors in evolution. The occasion will not permit me to do full justice to these subjects. That functionally-produced modifications are inherited was the great assumption upon which Lamarck founded his theory of evolution. Many able naturalists have insisted on it, and in my judgment there should no longer be any doubt whatever of the fact, not only so far as grosser structure is concerned, but brain structure likewise. No question is of more moment in the whole range of biology and especially biologic philosophy, and Spencer has well pointed out that on the answer to it will depend largely the sciences of psychology, ethics and sociology. Weismann, Lankester and others deny hereditary power in such modifications, the former believing that hereditary modification can result only from changes in the *germ plasma*, *i. e.*, are virtually congenital. Natural selection, according to this view, plays upon the germ plasma; but I have never been quite able to comprehend how this view, even if established, militates against the transmissibility of acquired modification, for, whatever theory of heredity we adopt, it shows us rather the manner of the transmission and therefore confirms its possibility. But the fact of such transmissibility rests neither on embryological nor theoretical grounds. It is a fact so fully demonstrated in the history of our domestic animals and the history of agriculture, that the skepticism of some of our great naturalists and embryologists must be attributed to that ignorance of the farmers' commonest experiences which is, unfortunately, a too frequent attribute of the city-trained investigator. Darwin in the beginning, and while the importance of natural selection was growing in his mind, allowed little importance to use and disuse for the same reason that he subordinated external agencies; viz., that, in proportion as it acts on masses simultaneously, it must diminish the importance of natural selection. Yet he allowed more weight to it toward the end and has furnished some of the best evidence drawn from domestic animals of the transmission of acquired characters, affecting the dermal, muscular, osseous and nervous systems. Spencer has shown that inheritance of functional modification is most easily observed and experimentally proved in those parts which admit of easy observation and comparison, as the dermal covering and the bones; and that they for the most part are beyond these tests in the muscular and nervous systems. Yet he logically concludes:

"Considering that unquestionably the modification of structure by function is a *vera causa*, in so far as concerns the individual;

and considering the number of facts which so competent an observer as Mr. Darwin regarded as evidence that transmission of such modifications takes place in particular cases; the hypothesis that such transmission takes place in conformity with a general law, holding of all active structures, should, I think, be regarded as at least a good working hypothesis."

So far as Entomology bears evidence, it confirms the fact that modifications of structure due to use or disuse on the part of the individual may be and are transmitted. These are easily observed in the exo-skeleton, and while the experimental proof is yet limited, it is not wanting, especially in the history of apiculture. Excessive use of any organ will develop or enlarge it at the expense of other organs, just as disuse will cause a diminution, or atrophy thereof. The variation in the individual will be within limits, but when once the variation has set in, the tendency is always to an increased variation in the same direction in the descendants, especially if they continue the same use or disuse. Here, again, however, it is difficult to separate the modification due to individual effort, or want of effort, and the more general modification affecting the mass of individuals of a species through the environment; because the environment affects function, and function in its turn affects form and structure. The life of every individual furnishes an excellent illustration of new action and new uses for organs not previously used, in the striking and sudden employment of postnatal organs, both of respiration and nourishment, which pre-natally had no corresponding action. Romanes has argued that *Cessation of Selection* may reduce an organ where use or disuse can have no play, as in the loss of wings in neuter ants; and that by the law of compensation an organ may even be increased, as in the heads of such neuters. He enforces the idea by exampling the blind crabs of our Kentucky caves, where the complex eyes rapidly disappear under cessation of selection, but where the persistence of the foot-stalks indicates that economy of nutrition could have had little play! It is difficult, however, to draw the line between this cause and Lankester's reversal of natural selection; and still more difficult to say wherein either differs from mere disuse.

Degeneration which has been urged as the true explanation of many of the existing forms of life is, it seems to me, but a consequence of disuse and would therefore fall into the present category, among causes of variation.

Emotion as affecting the Individual.—I have here considered the

factor of use and disuse as a direct cause of variation, from the psychical rather than the physical standpoint, *i. e.*, individual or conscious effort as furnishing food for natural selection, among more highly endowed animals, rather than as effort by species as a whole necessitated by physical conditions and inducing modification in masses irrespective of selection. This leads us to the consideration of mind as a factor in evolution, and we shall soon see its importance as a fundamental cause of differentiation, among higher organisms at least. I am not sure, even, that its influence can be excluded from among lower animals, however much we may have to exclude its action in so far as plants are concerned; for any new functional effort inducing new use may be looked upon as conscious and intelligent as compared with use fixed by habit and lapsed into automatic action or instinct. The former typifies variability and progress; the latter constancy and stability.

Mind is a comprehensive cause of variation and may be considered under several categories: We have, for instance, (1) the action of the mind of the individual in willing, or in selecting between differing alternatives that present themselves, as in the choice of means to ends; (2) the direct influence of the emotions on the individual; and (3) the influence of the emotions of the pregnant mother on her offspring.

In the first category the influence of mind in modifying is chiefly confined to man. It must have acted from the time when he first began to prepare his crude weapons of defence and offence to the present day, when some new discovery or some new invention may alter the map of the world, revolutionize society, or give one race or nation the advantage over another; nor can we feel sure that animals below man have not been modified by similar psychical effort. In the second category, the direct influence of the emotions on the individual, it is a psycho-physiological factor involved in the question of use and disuse; for if it be once admitted (and I think the tendency of modern neural science is in the direction of establishing the fact) that strong mental effort may be made to affect special parts of the body, *i. e.*, that an excess of nervous force brought to play on any particular organ or any particular part of the organism, induces increased growth or development of such parts; we can understand how far desire, especially under the spur of necessity, may be influential in inducing modification. Lamarck's idea, therefore, may not be so ridiculous as it has hitherto been

supposed by many. Darwin took no stock in this influence, and referred with some contempt to the views of Lamarck, and Geoffroy-St. Hilaire. He thought it strange that the author of "Les Animaux sans Vertèbres" should have written that insects which never saw their eggs should will them to be of particular form, which he thought hardly less absurd than to believe that the desire to climb should make a Pediculus formed to climb hair, or a woodpecker to climb trees.

Emotion of Mother as affecting Offspring.—There may be some doubt about the extent of the influence of the individual mind in inducing direct modification, for the subject is a difficult one to deal with and we have few exact data to draw from. Since in human affairs we recognize the power of will in affecting purpose and action and in moulding character, it is legitimate to infer that when our knowledge has increased we shall recognize its effect on function. There can be less doubt as to the third category, viz., influence of the mind or emotions of the pregnant mother on her offspring in inducing modification both physiological and mental. As a cause of variation, though believed in by J. D. Hooker, as we learn from the "Life and Letters," and by other of Darwin's contemporaries, it was discarded by Darwin himself, his principal reasons being that the results of observations made for him in hospitals were adverse to any such influence. Medical men, as a rule, also discard it as among the mere notions and superstitions of women, and argue its impossibility on the ground that there is no neural connection between mother and fœtus. The ancients practically recognized the influence of the imagination of the mother on her offspring, and belief in it is still very prevalent among women themselves, of all classes. Women alone are able to speak or feel in this matter, from experience, and the almost universal belief in the influence, among those who have any experience at all, should make us hesitate to discard it too summarily. From facts within my own personal knowledge I have long believed in this influence, and the more I have been able to collect reliable data bearing upon it, the more confirmed have I become in the conclusion that the emotional experiences of the mother affect the issue in varying degree, according to the intensity of the emotion. When sudden and excessive as in rage, fright, repugnance, etc., or where prolonged or accumulative, as in continued brooding, it may induce nervous disorders and even mental aberration, idiocy or insanity; or, again,

physiological change, as atrophy or increase of parts, and other peculiarities which have relation to the form or character of the inducing mental manifestation or shock in the parent. Investigation of this, as of all subtle phenomena, is attended with the difficulty of separating the chaff of fancy from the grain of reality. The method pursued by Darwin is unsatisfactory, as it dealt with normal conditions which furnish no evidence and with the fanciful or notional side of the subject. The literature of the subject is extensive and quite interesting, and I would refer particularly to the work and writings of Viellard, Schœnfeld, Demangeon, Lucas, Féré and Brown-Sequard. Two other difficulties confront the investigator: first, the somewhat unsatisfactory state of neurology and the difficulty of experimental research therein, as indicated by Vice President Bowditch before this section two years ago; *secondly*, the aversion, from feelings of delicacy, on the part of the persons concerned, to publicity of the more marked and striking evidence. The phenomena of hypnotism, proving as they do that physiological results may be induced through the imagination of the subject acted on by the mind of the hypnotizer, are suggestive in this connection, the work of Charcot in Paris more particularly showing how powerful the action may be and how the effects of actual medicines may be produced by the use of imagined ones. The mind of the hypnotized under these conditions is brought into those exceptional and exalted conditions which are necessary in the case of the mother to produce on her offspring the effect which we are discussing. The recent experiments of Mr. C. T. Hodge on the effects of stimulation on the nucleus and cell-body and on protoplasm are also interesting here showing, as they do, decrease in the two former and vacuolation of the latter as the result.

The history of science is present to tell us that common and persistent belief, based on experience, has not infrequently been met with skepticism and even ridicule on the part of scientific men, only to be vindicated finally by more thorough and exact knowledge. It is too often the case that, where the processes are recondite and difficult to follow, assumption passes for knowledge. The function of some of our own bodily organs yet remains to be established and we probably assume too much in requiring that all nervous force must be transferred through nerve tissue, or that there may not be protoplasmic filaments which are not resolvable, in their finer ramifications, even with our best microscopes. The

very nature of mind and its processes puts it beyond the reach of the scalpel of the anatomist or the physiologist, just as many psychical phenomena baffle the exact methods of science, at least those so far employed. Leaving out of the question the evidence of peculiar marks due to maternal emotion, cases of which are part of the unwritten history of almost every family, the striking cases of which I have authoritative evidence of addition to, subtraction from, or singular modification of, anatomical parts, confirm me in the belief that this is a most important psycho-physiological cause of modification.

In the romance of Elsie Venner, in which the heroine's strange attributes are connected with pre-natal influence of the mother, who died of the bite of Crotalus, Oliver Wendell Holmes has strongly put forth this doctrine in the form of fiction. I allude to this clever romance because of the medical knowledge of the eminent author, and because while admitting in the preface that a grave scientific doctrine lies beneath some of the delineations of character, he also affirms that he has had the most startling confirmation of its truth. The data collected on the subject I hope to bring together on some other more fit occasion, and I would take this opportunity of urging any in my hearing or who may read these lines, if they have had or are aware of any authoritative and illustrative cases, to communicate them to me with as much detail as possible.

This theory once established, its bearing on evolution as a prime cause of variation must at once be manifest; for it gives not only tangibility to the Lamarckian idea of desire influencing modification, but, also, a conception of how Infinite Mind in nature may act through the finite in directing such modification. No doubt but that there is a great deal of nonsense and superstition mixed with the genuine, and that the idea that every little whim, or fancy, or imagining of the mother will produce record, or mark, is one of the unjustified outcroppings of the fundamental fact, and helps to explain the difficulty of getting at the real facts and the ease with which Darwin rejected the idea. In my judgment this factor acts only when, from whatever cause, and particularly under the spur of necessity, the emotions are exceptionally intensified, or the desire strongly centred in some particular object. The conception is perfectly legitimate, for instance, that when a species is subjected to any external modifying cause, affecting all its members alike, the adaptive modifications which natural selection, under such circum-

stances would play upon, have their origin in the emotions, or the influences at work on the pregnant females, giving direction in their offspring, to the needed changes. In this way it is probable that only those individuals born under such conditions would be able to survive. Thus this becomes no mere ancillary cause of variation but one of deepest import and at the very foundation of evolution. The female in this light acquires an increased importance, and evolution finds her not only the essential at the dawn of life upon our planet, but, in its present highest manifestations she is nearest by instinct, intuition and aspiration to the Controlling Mind, which in the beginning quickened the great womb of nature and down through all the ages guided the continuous stream of life to designed ends through the individual womb of the mother.

As already remarked, the psychical factors which we have been considering are substantially Lamarckian, and in proportion as we consider them and get to understand the other direct causes of variation, must we give importance to the ideas of Lamarck and, conversely, less importance to the ideas of Darwin.

Did time permit I should like to go into an analysis of Lamarck's "Philosophie zoölogique" and show how the genius of this illustrious French naturalist anticipated a very large part of that which Darwin subsequently so laboriously helped to establish. I must pass the subject, however, and simply record my surprise that one who was otherwise so honest and fair toward other writers was so evidently unfair in his estimate of the work of Lamarck, as Darwin, in the "Life and Letters," is shown to have been. It is incomprehensible, reading Lamarck with our present knowledge, that Darwin should have found neither fact nor ideas in a book which abounds in both, except on the theory of a poor translation or that strange national antipathy which has so often prevented the people of one country from doing justice to those of the other, and which so long prejudiced the French Academy against Darwin's own especial theories.

Darwinism assumes essential ignorance of the causes of variation and is based on the inherent tendency thereto in the offspring. Lamarckism, on the contrary, recognizes in use and disuse, desire and the physical environment, immediate causes of variation affecting the individual and transmitted to the offspring in which it may be intensified again both by inheritance and further individual modification. Both represent important principles in evolution and

coöperate to bring about the results. The theory I propose gives renewed importance to the Lamarckian factors by showing one manner of their action not previously urged and it also helps us to a tangible and scientific conception of design.

Acceleration and Retardation.—In this rapid glance at the immediate causes of variation we have discussed some factors which, in some degree, represent laws rather than inducing causes of variation. This difficulty appertains to all attempts at formulation of the causes of variation, and only as our actual knowledge increases shall we be able succinctly and definitely to classify the factors. There are, however, certain important laws which have influenced modification but in no sense can be looked upon as causes of variation. They are laws or principles of evolution by which we may account for the formation of types, acting, just as natural selection does, in differentiating rather than in originating the variation. No one can have followed the important and suggestive works of Cope and Hyatt on the subject of acceleration and retardation and not feel that it expresses an important law of this kind. It is, as I understand it, a factor in evolution not comparable with the principle of natural selection, but complementary thereto, much in the same way as physiological selection and sexual selection are. It is an attempt to give expression and form to a set of facts to which palæontology undoubtedly points and which ontogeny substantiates, viz., that certain types may attain perfection in time and then retrogress and finally become extinct, and that existing types which are dying out, or degenerating, exhibit, ontogenically, the culmination of force and complexity, followed by decadence, corresponding to the phylogenic history of the type. We know, from the "Life and Letters," that Darwin gave up in despair the attempt to grasp the full meaning of these particular views of our associates, and in a letter to Hyatt, with characteristic modesty, he attributes this inability to his own dulness rather than to any weakness in the theory. Others have experienced the same difficulty and believe, with Professor Morse, that the facts enumerated, as well as the facts of exact and inexact parallelism are explicable on the doctrine of natural selection. This is true, it seems to me, only on the broader, unjustified interpretation of the doctrine to which I have previously alluded in the opening of these remarks. The law of acceleration and retardation may, perhaps, be substantially stated in this wise: that certain groups acquire some charac-

ters rapidly, while corresponding groups acquire the same characters more slowly, or never acquire them at all, and this brings us to another important factor of evolution which serves to give force to the law.

Acceleration by Primogeniture.—This has been elaborated by Hubrecht. He argues that so long as the parent form remained most in harmony with the surrounding conditions it would maintain in the struggle for existence its characteristics against all tendency to vary in its offspring; which is equivalent to saying that it will remain unchanged so long as the environment remains the same. He then shows that in organisms in which the reproductive period covers many years, accelerated development by primogeniture, *i. e.*, as between the first born and the last born of any pair and of their posterity, will, in time, produce differentiation. The series of the first born will, in the course of time, involve many generations at short distances from each other, whereas, the series of the last born will, on the contrary, consist of a much smaller number of terms each separated from its predecessor by a more considerable distance. Any tendency to variation from external or internal influences must needs find more numerous occasions to act in the series of the first born, not only because these have a more composite ancestry but because they necessarily become the most numerous. In other words, the chances are more numerous for small differences among the first born series, and in proportion as such differences are accumulated, intercrossing and bastardizing with the series of the last born will become rarer. This law will gain from physiological selection and, it seems to me, throws additional light on that of acceleration and retardation. It must act more particularly among higher animals where the reproductive period is lengthened and the time between the first and last born is great.

Saltation.—We are thus led to what have been called saltations in evolution. Although the history of palæontology has continually added to our knowledge of past forms, and helped to fill up many gaps in the evolutional series, and although, during the last quarter of a century, it has particularly vindicated Darwin's prophecy that many links would yet be found, the substantial truth remains that gaps still occur, and that progress, so far as present knowledge indicates, has been made by occasional saltations. There have been, it would seem, periods of rapid movement, and of comparative repose, or readjustment of equilibrium. Cope con-

cludes that, "genera and higher categories have appeared in geologic history by more or less abrupt transitions or *expression points*, rather than by uniform gradual successions."

One of Pictet's strongest points, in opposition to Darwin's theory, which struck Darwin himself with much force, was that it ill agreed with the history of organisms with well marked and defined forms, which seem to have existed during but a limited period, as for instance, the flying reptiles, the Ichthyosaurs, Belemnites, Ammonites, etc. Some authors, who have fully recognized these gaps or leaps in the developmental history of animals, yet believe them to be consistent with the theory of gradual modification. It may be only one individual of many which becomes modified and transmits the modification to descendants: it may be but one species of a genus which, for similar reasons, supersedes the rest which become extinct in time proportioned to prolificacy.

There is no reason to suppose that the history of organic life has differed in this respect from that of inorganic. We need not discuss here the question of catastrophism and uniformitarianism in geology. However much the latter prevails at the present time, both have doubtless operated in the past. Catastrophism would necessarily produce gaps, or saltations, in the palæontological record, as only the more plastic species would adapt themselves and survive under its influence. It is not gaps due to such causes that are here to be considered, however, but those which occur in uniform strata. Haldeman has most suggestively remarked that the same mineral will crystallize with three, six or twelve angles, but not with five or seven, and he asks: are the facts of organic morphism subject to less definite laws? Cope has drawn another illustration from inorganic forces, in the three great changes in water, from solid, liquid and vapor, which take place suddenly at what may be called three *expression points* of the thermometer, the many intervening degrees involving no change. Rhythm or wave movement would seem to be a universal attribute of matter, whether organic or inorganic. The forces of nature are constant, but the phenomena induced are often paroxysmal. The progressive forces accumulate, while the conservative forces resist until at last resistance gives way with comparative suddenness. There is every reason to believe that the life movement, in its ascending complexity, has shared this common law. Accumulation is proportioned to the change in environment, and resistance to the age or rigidity of the

organism. The latter may be strong enough to end in death or extinction, or it may break down and, with comparatively sudden yielding and conformity to necessity, burst the confines, and begin a new series of variations and adaptations. In either case we have breaks, because the dying or dropping out of one type makes room for another, more accommodating. Rapid evolution, from causes already discussed, implies gaps which must be marked according as the strength of the conservative forces and the violence of the final accommodation are great, and because sudden breaks are more apt to occur after long periods of stability. The break may be induced by changes in physical environment or without such change; if the latter, it will more likely occur in some individual, born with a marked departure from the type that gives it some advantage, and whose issue will in time supplant all other individuals. In either case, we shall have, palæontologically, distinct species or genera, one superposed on the other, without links. To the imperfection of the geologic record is to be attributed, no doubt, a large number of these gaps yet existing between types, and many important links, or branches, are yet to be discovered. Yet the views we have been considering should absolve evolutionists from all necessity of demonstrating the more minute gradations; because, in deposits like the Tertiary, during which we may assume life-conditions to have remained comparatively uniform, these saltations take place. Saltation, or, what is probably a truer expression, wave-movement, would indeed seem to be a prerequisite of progress and will account for much that is going on even at the present day. In artificial selection by man we find that it is at first comparatively easy to accumulate minute peculiarities and variations by rigid breeding and exclusion of all deviation; but that we soon arrive at a fixed point which is maintained at first with difficulty but with increasing ease with each generation. During these more fixed periods the potentiality for change is doubtless increasing, until at last it is suddenly manifested in renewed variation. Rest is followed by activity just as surely as activity induces and requires rest.

There is a limit to development in organs, just as there is a limit to individual mental growth. Weariness of previous effort comes upon us when the limit of result is attained, accompanied by great longing for change and not infrequently with revulsion from previous effort. The naturalist who has devoted a part of his life to

the persistent accumulation of facts and specimens, has held the imaginative and generalizing powers in abeyance during that period. The reserve brain force in this direction may be suddenly called into activity by exhaustion in the other, and the process may perhaps be comparable to the exhaustion of the soil for one particular crop, without lessening its fertility for some other, the recognition of which fact is the foundation of all successful agriculture. Excess of development, whether in body or mind, inevitably brings about either wholesome reaction or utter collapse.

How far the rhythmic tendency in the development of animal life may be explained by the rapid change of climate, by migration and the loss of record, or upon the general law that while there has been progress of the whole, there has not necessarily been progress of every part, it would take us too far to discuss in this connection. I think we are safe in saying, however, that the facts justify belief that, in the evolution of animal life, as in the evolution of everything else, progress has often been made by waves.

The Fiskean Law.—With regard to what may be called the Fiskéan law of correlation between brain development and infantile dependency, Fiske has so admirably elaborated the subject that it needs no further elucidation here as the principal factor in the evolution in man, first, of the family relation, then of the clan, the tribe and the nation. With this factor in mind, and the immense superiority which anthropoid man must have had, when brain development had once induced this fundamental community of interest, over the rest of brute creation, the gap between primitive man and the higher anthropoid apes in the past or between the present lower races of man and the higher existing primates is easily explained, even if it had not been greatly exaggerated. At the present time we may note and record the further inevitable increase in the gap, for the lower races of man are gradually becoming extinct and the higher apes cannot long hold their own or persist.

The Brooksian Hypothesis.—I have already alluded to Brooks's hypothesis under the head of sexual differentiation, and his work on Heredity must be so familiar to you that his views need but a passing notice. He believes that sex differentiation means fundamentally a physiological division of labor and that the male is essèntially the progressive or diversifying and the female the conservative agent. As organisms gradually increased in size, as the number of cells in their bodies became greater, and as the differ-

entiation and specialization of these cells became more and more marked, one element, the male cell, became adapted for storing up gemmules and, at the same time, gradually lost its unnecessary and useless power to transmit hereditary characteristics.

The theory finds support in some of the phenomena of life and doubtless expresses a law not easily established, for which reason it will not be readily accepted. It leaves entirely out of consideration some of the forces at work which I have already indicated and in so far must be considered only a law of secondary importance. However much we may admit the general truth that the germ cell continues the past and the sperm cell tends to diverge from it, as a purely dynamic proposition, inducing variation for natural selection to play upon, it does not in any way decrease the overwhelming importance of the female in inducing, through psychico-physiological influences, a needed and purposeful modification in the manner which I have already expounded.

Having thus considered some of the proximate causes of variation and some of the more general laws of evolution we are naturally led, in conclusion, to consideration of original or Infinite Cause. Far be it from me to try your patience with any prolonged speculation upon the more profound problems of life and of futurity which have been dealt with by able men of all times and with such conflicting and varying results. I shall content myself, in closing, with a few words upon those themes which, as biologists, we cannot ignore, and to which the subjects we have been considering inevitably lead.

Mind, as exhibited in organic evolution, however simple or complex may be its manifestations, is, in essence, one and the same force. There is an undoubted gradation from simple sensitiveness and volition, to the more complex instinctive and reasoning faculties of higher animals. The concensus of opinion of biologists who have given the subject any serious consideration is that animals exhibit all our mental endowments in degree. They possess desire, affections, imagination, memory, comparison, judgment, and all the other attributes of human intellect, more or less perfectly developed, and there should be no more doubt of the gradual evolution of man's intellect from preëxisting lower and simple forms of aggregate mind, than there is in the derivation of his more complex and powerful brain (the medium of mind) from smaller and simpler brains, or of his body from simpler, less specialized forms.

Some of our profounder thinkers feel that all the phenomena of the universe are but forms of motion, and that the great final and creative force may best be imagined as the centre and the source of motion. In this light, it is, perhaps, not too much of an exaggeration to conceding to plants a certain amount of mind, and in establishing the fact of universal movement in plants; in showing that circumnutation is an inherent quality or manifestation of plant life, Darwin may well be said, as Professor Ward has expressed it, "to have gotten at the final cause of all evolution." Indeed, it must be difficult for a botanist, considering that all the terminals of a plant, whether beneath or above the ground, are constantly feeling and choosing; or when he reflects on the wonderful motions and *selecting* power of climbing plants, but more particularly on the strange phenomena exhibited by insectivorous and entomophilous plants; it must be difficult for him, I say, to relegate to the limbo of the unconscious, the whole of his pet kingdom. In his letters, especially to Hooker, Darwin says, speaking of the circumnutation of *Echinocystis lobata:* "The tendrils have some sense, for they do not grasp each other when young," and again of the movements caused by light, gravitation, etc., he says: "It has always pleased me to exalt plants in the scale of organized beings, and I have therefore felt an additional pleasure in showing how many and what well adapted movements the tip of a root possesses." In studying the movement of flowers, especially in their sexual relations, I have often felt, with the poet that:

> "'Tis my faith,
> That every flower enjoys the air it breathes"

and that, however much conscious sensation may have lapsed in most of their manifestations of growth, sensation may still be conscious, and mind still work, in dim fashion, during the season of reproduction, in the specialized and reproductive parts. It may be a fancy, but who can prove it unjustified?

Where, then, shall we draw the line in the evolution of mind between the high degrees of consciousness in animals and self-consciousness which is believed to be a peculiarly human attribute and at the foundation of all that constitutes con-science and makes him a moral and responsible being? The beginnings of self-consciousness are traceable in animals, since many of the phenomena of sexual selection and the well known sense of shame in our domes-

tic associates could scarcely have resulted without it, and it seems to me illogical to argue, as some of our best writers on evolution have done, that self-consciousness is an attribute that must have been breathed into man by special, supernatural act.

From the consideration of the general subject of mind in nature, we are brought inevitably to the question of Design. There can be no doubt that the tendency of evolution has been to remove further and further the idea of an Infinite First Cause, and pure Darwinism exhibits to us a cold and cruel world,—exemplifying the Hobbesian theory of self-love, nothing having any reason for existence except its own welfare. It leaves out all the higher beatitudes of nature, the higher aspirations of men, and all those internal yearnings or laws of internal growth and influence, not for the individual alone, but for the good of the whole. Natural selection, while not disproving Design, pushes back the argument and limits it to the order of nature as a whole, since much of the modification and adaptation is accounted for by known and recognized physical conditions. The argument for design, however, as Asa Gray has so well set forth, rests on the fact that the designed and the contingent can never be accurately discriminated and that limitation, in the very nature of the case, is inconceivable. There are those who see only in nature the inevitable and necessary manifestation of the forces of the universe, and assume them to have been without beginning and to be without end. There are others who find in the unity—the oneness—of the forces of nature full evidence of Design. So far as logical inquiry will aid us, the recent controversy between Messrs. Cope and Montgomery may be cited as typical of that which has always separated the two schools, and sufficiently shows how opposite conclusions may be reached by those equally able and equally conversant with the evidence. The former contends that mind exists independent of matter, or, to use his own words, "primitive consciousness exists in primitive forms of matter and constitutes a primitive person, or Deity;" while the latter maintains that the mind of living beings is, itself, only a product, or outcome of their organization.

Darwin, as is evident from the "Life and Letters," was induced by his life-work gradually to abandon the rigid tenets of the Christian church, and to substitute therefor a more latitudinarian form of belief. He was content to limit his investigations to truly scientific methods, realizing fully that all speculation in reference to the

great First Cause involves the discussion of the supernatural which is necessarily beyond the natural. The discussion of these questions can be very little affected by the methods of natural science and the more thoroughly we recognize this fact the better it will be for both religion and science. It ill becomes us to be dogmatic in questions beyond the limits of induction, or where the mind attempts to fathom by deduction and speculation. Natural science necessarily deals with natural causation.

A few expressions from the "Life and Letters" may be quoted here. Darwin says: "The old argument of design in nature, as given by Paley, fails now that the law of natural selection has been discovered. We can no longer argue, for instance, that the beautiful hinge of a bivalve must have been made by an intelligent being, like the hinge of a door by man. There can be no more design in the variability of organic beings and in the action of natural selection, than in the course the wind blows." He asks whether we are to believe that organic forms in nature are preordained. If so, why should we not believe that the variations of domestic animals and plants have been preordained for the sake of the breeder; yet if we give up the principle in the one case we must do so in the other. Again: "No shadow of reason can be assigned for the belief that variations in nature have not been the result of the same general laws which have formed the groundwork, through natural selection, for the formation of the most perfectly adapted animals in the world, man included, without intentional or specific guide." Yet, while he was willing to remain an agnostic, feeling that the beginning is insoluble by us, he gave some of the most cogent reasons, aside from the feelings and emotions, for the belief in the existence of a First Cause—of a God. In his correspondence with Gray, he shows how he cannot bring himself to believe that a beneficent and omnipotent God could have designed the characteristics of parasitic and carnivorous creatures, with all the other heartless manifestations in nature. Yet he was not content, in contemplating the nature of man, with the idea that everything is the result of blind force. He believed in design with the details, whether for good or bad, left to the working of what, for want of a better term, we call chance. With the utmost candor, he confessed that this view was not at all satisfactory to him, but that the whole subject was too profound for the human mind, likening our speculations upon it to those of a dog on the mind of Newton, and believing

that each man should hope and believe what he could. Again, in a letter to Lyell, June 4, 1860, he remarks, "As squared stones, bricks or timber are indispensable materials for a building and influence its character, so is variability indispensable in influencing selection;" and that, "in the same manner as an architect is the all-important person in the building, so is selection with organic bodies." The parallel is evidently weak here and we may well insist that from Darwin's own statement, the idea of an intelligent architect or designer in the one case is as all-important as in the other. The same is true to some extent of the argument from artificial selection, or the manner in which *intelligent* action in man acts on domestic or cultivated organisms. This is not limited to the mere choosing of chance variation in beneficial directions, but involves also purposeful feeding, training and treatment, to induce the desired amelioration and variation. Just as the production of new forms among domestic animals or cultivated plants implies a purpose—a designer; so may the origination of such forms in Nature imply intelligent design. Both Lyell and Gray believed in the form of variation having been planned or designed. It seems to me that the evidences of design in nature are so overwhelming that its advocates have an immense advantage over those who would discard it. A fortuitous cosmos is, to most persons, utterly inconceivable; yet there is no other alternative than a designed cosmos. To accomplish anything by a process, or by an instrument, argues greater, not less power, than to do it directly, and even if we knew to-day all the causes of variation, and understood more thoroughly than we do the method of evolution, we should only carry the sequence of causes a step farther back and get no nearer to the Infinite or Original Cause.

The most philosophic view is, probably, that which, while recognizing an intelligent creative power, or mind, which has worked and is yet working through ordained laws, yet leaves the detailed manifestations to secondary causes and finite action. Limiting conditions, or laws, since law is but a limiting condition and nature an active power, may act together in producing secondary causes but the great and Infinite Cause may be looked upon as that which upholds the universe.

The tendency of those who would advance theological conceptions from evolutional grounds is doubtless to some monotheistic form,

or a sort of psychical pantheism, which sees the Infinite immanent in Nature — an all-pervading, interpenetrating Essence. This is the All-conscious, and I know of no better intellectual conception of immortality than resolution of the finite individual life and conscience into the All-life, All-conscious — a conception justified and affirmed as it is by evolution. But the one great point which I wish to emphasize in alluding to these differing views is, that, beyond the limits of the scientific method, speculation becomes *faith* and the most profound writers are obliged to use the simple *I believe* which most theologians begin with. The sentiments which constitute true religion — the sense of impenetrable mystery and moral obligation; the higher humanitarian promptings and hope of future life,— are apart from science and cannot, therefore, be affected injuriously by its methods.

Evolution logically involves the idea of the origination of life from the inorganic forces at work on our globe and only in this sense does it modify our conception of the Creator. The one vivifying force in nature must needs be an endless source of power and movement, with the potency of all that is in what we call universe. Art and plan being manifest therein the power must needs be intelligent, and we can compare it with nothing more satisfactorily than with our own conscious thinking power, whether we call it soul, spirit, the Absolute, the Infinite, the Unconditioned, the Unknowable, the Creator, the Divine Mind, the Supreme Will, God, or by what other *nominis umbra* we choose to express the conception.

And now, Ladies and Gentlemen, wisdom admonishes me to be brief. I have ventured just within the question of design because of the prevalent belief that evolution eliminates it from our conception, and because I have felt that as between the extreme schools the middle ground chosen by our late lamented Gray, is far the more satisfactory and philosophical. On the other great question of what life is, or how it originated, I commend the candor of Marsh in closing his address as president of the Association in 1877 with the words "In this long history of life, I have said nothing of what life is. And for the best of reasons, because I know nothing."

If we endeavor to formulate the idea of life we shall not lack for excellent guidance in past utterances of those who have been and who are yet members of this section of the Association. In fact we may well feel proud to live in the generation which has pro-

duced some of the ablest experimenters whose work will have enduring influence in bringing us nearer to a true comprehension of the forces under which we live. Professor Barker, in his presidential address for 1880, on "Some modern aspects of the life question," seemed to some of us on the verge of chemically and physically explaining and defining the thing itself; and, yet, though we seemed, while listening, to near the gates which opened the secret we got but a glimpse into the great mystery. It matters very little whether we follow the great writers who have wrestled with this question in the past, or those who have been or are still with us, we must acknowledge our utter incapacity to comprehend the most patent phenomena of our being. We try in vain to analyze life; we try equally in vain to measure thought.

A little transparent colorless material christened protoplasm is believed to be the physical basis of life and the seat of all those marvellous powers and properties belonging thereto. We may trace back from death and dissolution the individual life to its very genesis in the organized cell, or ovum; we may go farther back, and trace it to the unorganized nitrogenous hydro-carbon compound which has been named protoplasm; but we yet fail to apprehend the cause of its vital functions, of its powers of assimilation, irritability and reproduction—the potentiality of the living as compared with the dead stuff. So in the past history of the type, we are led back to the same mystery. Ontogeny and phylogeny alike lead us to the riddle of the beginning of life. Huxley may well say that the process of development of the egg, like that of the seed, is neither more nor less mysterious than that by virtue of which the molecules of water, at the freezing point, build themselves into regular crystals. In either case it must be very largely a question of temperature. But while in the case of water the effect is at a fixed degree, in the case of organic life the degree is variable. We may say, with one of our most profound and respected writers, that organic compounds are "substances whose highly complex and very unstable molecules are composed themselves of inorganic compounds or of organic compounds of lower organization formed on the cool surface of fully developed plants at life-supporting temperatures." We may assume that increasing complexity and instability of nitrogenous substances give us proteids and finally protoplasm. We may find that the forces upon which the movements and changes

of this substance depend are the very same which determine those of a drop of water or a bit of mucilage; that all its movements are referable to the mechanics of molecules as exhibited in liquids. But the fact remains that the physicists who help us to a better understanding of the material properties and processes of protoplasm are only dealing with the methods and no more solving the great mystery of life than did Darwin or any of the other workers who have helped us better to comprehend its processes.

It was my privilege in years gone by to meet frequently with a little coterie of metaphysicians in St. Louis which included men like Wm. T. Harris and Thos. Davidson, who have acquired national reputation. With strong inductive tendency of mind I was all the more anxious to follow their erudite discussions of the philosophies of Aristotle, Kant and Hegel, but was too often overwhelmed with the feeling that my friends were dealing in mere words and that philologic juggling too often passed for reason. May we not truly say the same of those biologists who endeavor to give us a purely physical or materialistic explanation of life? I have certainly sometimes felt, in following their writings, that we scarcely get away from the thraldom of mere words. I felt this strongly, again, while listening to the symposium by a number of our greatest authorities (Schäfer, Lankester, Krause, H. M. Ward, Carnoy, Hartog, Gardner and Sedgwick) on the present aspects of the cell question at the meeting of our sister society in Manchester last autumn; for if there was one result which clearly came out of the discussion it was that protoplasm is not a mere chemical substance but a complex structure. Yet what stupendous import has been given to it in biologic literature, since Von Mohl first used it to indicate the slimy substance within the vegetal cell and Huxley aggrandized it as the "physical basis of life." Neither it, nor Lankester's plasmogen, nor Weismann's idioplasm, nor Coues's transcendental biogen, helps to any more real conception of life itself than does Hinrichs's pantogen which he calls the "primary chemical principle or the element of elements," and upon which he bases his theory of ato-mechanics.

The genesis or formation of individual life, in spite of saint and sage, is yet a mystery and probably always will be, and the exact site of the origin of the reproductive cells, according to one of our best authorities (Allen Thompson), is even a matter of doubt.

. . . .

"Hunt as we will all matter to the end,
Life flits before it: last as first we find
Naught but dead structure and the dust of fact;
The infinite gap we cannot apprehend,
The somewhat that is life—the informing mind."

All that evolution recognizes is the transmutability—the generic identity—of the forces of Nature, which, in their aggregate action, may properly be defined as omnipresent energy. We know, as a matter of the simplest observation, that this combined force, or energy, is essential to the continuance of life, not only upon our planet, but, deductively, in the universe. We are justified in inferring that it is capable, under fit conditions, of originating life, from what we know as non-living matter. Evolution, in fact, inevitably leads to the inference that vital force is transmutable into, and derivable from, physical and chemical force. It implies, and necessarily implies, abiogenesis, either now or at some remote period in the history of our earth; and notwithstanding experimentation and advance in our knowledge of micro-organisms overwhelmingly refute Bastian's conclusions, or the idea of the present origination of organic from inorganic matter, so far as forms within our ken are concerned, yet it seems to me perfectly logical to infer a world of beings which our strongest instruments fail to reveal to us, among which abiogenesis is possible to-day or archebiogenesis was in the past, just as the molecule is a necessary physical conception though it may never be revealed to us.

It may be true that phylogeny postulates the derivation of all forms of life from a similar simple cell and that, to use Joseph LeConte's language, "the fundamental law of embryology teaches us that the history of the phylogenetic evolution of organisms is mirrored in miniature in the ontogenetic development of each individual;" but in ontogeny the simple cell from which the individual starts has a potentiality, a something which makes it different from another and it does not help us much to have this something called the *memory of the plastidules*.

We have thus been led, while considering the causes of evolution, to the conviction that they are in the end referable to an Infinite First Cause and that evolution in nowise dispels the idea of a Creator, or need in any way affect religious sentiment; for, while it destroys much of the folly and superstition that belong to the ideas

which grew out of the false notions of cosmogony prevalent in the early infancy of the race, it points to higher and nobler forms of religion and to a better code of morals. Religion is born of the "delirious yet divine desire to know," and largely grows out of unexplained phenomena. Evolution is a principle which to-day pervades and rules the whole realm of philosophy. It establishes a noble conception of Divine nature, and exhibits God in his works. No longer "one touch of Nature" but, essentially, "Evolution," "makes the whole world kin." It is a bond that holds all life together. It shows us man as a part of the life everywhere about him, and promises advantage to him in more intimate knowledge of that life from which he used to think himself entirely severed by Supreme will.

Evolution teaches that nothing is yet so perfect but it may be improved; that good comes of the struggle with evil and the one can never be dissociated from the other. The erect position which has given man his intellectual preëminence, has brought him manifold bodily ills. No evolutional sibyl looks to a milennium. Higher development must ever mean struggle. Evolution shows that man is governed by the same laws as other animals. Environment has induced some of his most marked race characters, and individual variation is greater in him than in most creatures, and this is his surest promise of betterment. The dime-museum is eloquent of the fact that were any bodily variation useful, from an elastic skin to a hundred and one abnormal qualities, selection would have no end of variation to choose from. Differentiation has gone on and is going on to make him the most heterogeneous in function, whether of brain or muscle, of all species. The difference between the hand of a Josef Hofmann and of the average plowman is one which involves a large number of correlated differences and is functionally, if not structurally, greater than that between the non-opposability of man's great toe and its opposability in the higher apes—a difference which has been so much made of but which finds its gradations in eastern peoples who use their toes in "handling" tools.

Evolution is particularly promising to us as a people.

> "The spirit grows with its allotted spaces;
> The mind is narrowed in a narrow sphere."

The diversified environment and the commingling of so many

nationalities have stimulated variation and progress, making us already foremost in invention and material growth. The distinctive American type, not yet fixed but forming rapidly, must profoundly influence man's future on the globe, and evolution teaches that by so far as the contest is severe with those forces which are disintegrating and destructive to liberty will the final improvement be greater. The effort and advancement of to-day, in all directions, will form the inheritance of future generations.

But the struggle for existence in civilized man is no longer one solely for physical adaptation to natural environment or physical supremacy, but is one rather of ideas and purposes. It goes on in an environment of intellect and morals.

In addresses before this Association, Professor Morse has eloquently alluded to some of the more practical teachings of evolution as they should (and in the future doubtless will) affect governmental and political action. The higher forms of human law and order acquire meaning when we realize that crime is largely the persistence of ancestral traits, and that our laws are best when best suited to eliminate these barbarous propensities from the race. We can scarcely do our duty even to the criminal without a due comprehension of the power of heredity to jump back to an ancestral copy even in minutest detail.

The old forms of theology have encouraged a species of parasitism by denouncing man, as a whole, as condemned to eternal damnation—as inherently wicked by inheritance—and by offering him salvation through the help of others. Evolution teaches the care and preservation of the body and not its sacrifice; it teaches that right-living and right-doing ennoble; that inactivity, parasitism, viciousness of every kind, are degenerative forces in bringing disease and suffering and retribution. It shows that the worst ills are part of a legacy which no court admits to probate, but which nature executes relentlessly. It teaches the importance of proper environment and influence, especially in childhood, in order to improve what we may inherit for good, and counteract what we may inherit for bad. It shows the necessity of establishing the habit of right-doing and right-thinking where the old systems neglected both body and mind and relied on preaching and punishment. It teaches that our moral and penal codes should be so framed as to prevent the propagation of diseased and vitiate constitutions. It teaches that every increasing advance follows use and effort, while

degeneracy follows disuse and stagnancy. It teaches that man has risen from a lower, not fallen from a higher state; that from simple social conditions complex ethical conditions have resulted. It teaches that no work is complete and posits of man unlimited advancement in the future; for every advance in knowledge and every new application of science increase his moral power.

Ecclesiasticism lands the naturalist in skepticism and doubt of all that has been taught him as to the past and the future. Evolution reveals a past which disarms doubt and leaves the future open with promise—unceasing purpose—progress from lower to higher. It promises higher and higher intellectual and ethical attainment, both for the individual and the race. It shows the power of God in what is universal, not in the specific; in the laws of nature, not in departure from them. It may lead to some modification, as compared with Judaism, of the ideas of the future as it has of the past; for if the possession of the higher attributes which we denominate by the term Soul is the best promise of immortality, I believe there are many dumb creatures who are surer of it than many human brutes. All the word-moulding; all the rhetoric; all the sophistry, of those who, cradled in Mosaic theology but graduated in evolution, attempt to frame from the teachings of this last a post-mundane heaven of unalloyed joy for man alone, must in my judgment, come to naught. Their efforts remind me of the reconcilers whose business is, as Huxley has so well put it, "to mix the black of dogma and the white of science into the neutral tint of what they call liberal theology." Let us not deceive ourselves! What we accept as to the resurrection of the individual is based on other evidence than that of evolution, and is mainly a matter of *Faith*, and when it comes to forms of Faith, those are best which best subserve the moral and intellectual growth and development of society and which at the same time bring comfort and hope to the individual. The few great beliefs which have controlled the religious sentiment of the world have all helped to those ends and have been good in their day and clime. The teachings of Christ, in their simpler and purer inspiration, and freed of the narrowing incrustations of schism and dogma, transcend them all, and are, in fact, an evolution from them. Our faiths will vary as they have varied. Those who have attained to altruism may find sufficient joy and reward in present existence with its love and duty and conscious self-development, and rest

satisfied to leave to destiny the future after death; to candidly avow them ignorant of it —agnostics. Others may feel no regret in the conviction that there is no continuity of state but only of being; that eternal unconsciousness—eternal rest—awaits the close of individual life. But we should not forget that the mass of mankind are incapable of like unconcern, and that a Faith to them is precious and even necessary. Nor should we forget that the evolution which we, as individuals, have undergone, with all its risks and dangers, awaits every new individual born. The child up to a certain age, and the mass of mankind at maturity are, in apprehension, only as our savage ancestors, and must be taught the truth according to their light. The experience gained by those who have reached the highest ethical and intellectual growth must be formulated in precept and principle to be of any benefit to society at large, and the higher ethical sentiment and religious belief—faith, love, hope, charity—are priceless beyond all that exact science can give it.

PAPERS READ.

DISCOVERY OF THE PRODUCTION OF IMMUNITY FROM CONTAGIOUS DISEASES BY CHEMICAL SUBSTANCES FORMED DURING BACTERIAL MULTIPLICATION. By Dr. D. E. SALMON, U. S. Agricultural Department, Washington.

AT the meeting of this Association held in Buffalo in August, 1886, I had the honor to read before the Biological section a paper entitled, "Immunity from Contagious Diseases produced by products of Bacterial Multiplication," and a second paper on "The Theory of Immunity from Contagious Diseases." In the first of these papers I gave a detailed statement of nine different experiments in which forty-five pigeons were used. The results of these experiments demonstrated incontestably that these birds, which were susceptible to the dose of ¾cc. of hog cholera virus, might be made immune from this dose by hypodermic injections of sterilized culture liquid in which the germ of this disease had been previously allowed to multiply.

Nearly a year and a half after the reading of this paper (Dec. 25, 1887), MM. Roux and Chamberland, of Pasteur's Laboratory, published a paper in the *Annales De L'Institut Pasteur* in which they gave experiments to show that guinea pigs may be made immune from the effects of the *vibrion septique* by inoculation with sterilized culture liquids in which this germ had been allowed to multiply previous to sterilization. In a footnote to this paper they say, "At the moment of going to press we read in the Annual Report of the United States Department of Agriculture, that M. Salmon has succeeded in giving immunity to pigeons from hog cholera by injecting into these animals sterilized cultures of the microbe of this disease."

In an article in the *Annales De L'Institut Pasteur* of February 25, 1888, Roux publishes another article showing that guinea pigs may be made immune against the virus of symptomatic anthrax by injections of cultures in which had been grown the *bacterium chauvœi*, which cultures had afterwards been sterilized by heat. In the same number of this periodical MM. Chantemesse and Widal publish an article on "Immunity against the Virus of Typhoid Fever by soluble substances." In this article they make the following statement: "In 1886 Salmon rendered pigeons refractory to hog cholera by inoculating with sterilized cultures of the microbe of this disease The experiment was of little weight [*était peu concluante*] because the pigeon is endowed with a feeble receptivity for this virus and because the process of vaccination applied to pigs and other mammals completely failed. . . There was wanting an incontestable experiment which showed immunity given to an animal species very sensitive to the virus by means of soluble

substances secreted by this virus. This experiment and this demonstration are found in a memoir that MM. Roux and Chamberland published in the month of December in the *Annals De L' Institut Pasteur* on the 'Immunity against Septicæmia.' This work inaugurates a new era in bacteriology."

Hueppe in an article in the *Fortschritte der Medecin*, for April 15, 1888, takes up the history of the investigations of this subject properly giving credit to my experiments, the first of which were reported conjointly with Dr. Smith before the Biological Society of Washington in February, 1886. He shows that the objection of Chantemesse and Widal is without foundation, because of the check animals used in our experiments.

There is still a determined effort being made, however, by the assistants of Pasteur to secure the credit of this discovery for the Pasteur laboratory. In a review of my investigations of hog cholera written by Duclaux and published in the *Annales De L' Institut Pasteur* for July 25, 1888, the subject is again taken up and a labored effort made to show that my experiments were not conclusive. M. Duclaux begins his review by saying: "The works of M. Salmon on hog cholera are scarcely known in France and appear to be known little better to the European public. There are two reasons for this. The first is, that they have been published in an official report not found in commerce, and the copies which reach Europe generally go to be buried in the tomb of the ministerial archives, or in the grand mausoleums hostile to visitors that we name, in France, public libraries. The other reason, more personal to the author, is that he proceeds too much by that method of exposition which consists in pouring on the head of the reader the entire contents of his notes of researches, leaving him to make his choice among the materials which thus reach him without order and without measure. . . . . . As I said in the beginning, they are not known in Europe where they scarcely figure in the journals until after the tardy communication which was made in 1887 by MM. Salmon and Smith to the Washington congress. But had they been known sooner, they would not have been sufficient to rally the then hesitating opinion on the subject of vaccination by soluble substances. They exposed the flank too much to very legitimate objections."

Before considering the objection raised by Duclaux to the experiments themselves, I desire to call the attention of the association to certain publications in which these experiments were referred to at considerable length prior to the meeting of the International Medical Congress at Washington. The first communication was read before the Biological Society of Washington, Feb. 20, 1886, and printed in volume III of the Proceedings. This paper was printed in full in the American Veterinary Review for May, 1886, and it was referred to at considerable length in an editorial printed in the New York Medical Journal of March 6, 1886. This paper contained our first experiments with sterilized culture liquids and in it was given the demonstration of the principle, that immunity may be produced by the injection of soluble substances which result from the growth of pathogenic bacteria. The paper was confined to one topic, was concisely written and occupied less than five pages, and consequently is hardly

open to the criticism of M. Duclaux. The paper read before the American Association for the Advancement of Science in August, 1886, was printed in full in the Proceedings for that year, p. 258, and was also printed in the Medical News of September 18, 1886. It appears to me therefore that if the scientists connected with M. Pasteur's laboratory were ignorant of these investigations until after the meeting of the International Medical Congress in September, 1887, it is not because the literature was inaccessible.

MM. Chantemesse and Widal take exceptions to my experiments because pigeons, which were the animals used, were but slightly susceptible to the virus, and because this process of vaccination applied to hogs and other mammals had completely failed. It is somewhat amusing to find in the same number of the *Annales* a communication from M. Roux, of M. Pasteur's laboratory, claiming to demonstrate that immunity can be produced from *Charbon Symptomatique* with soluble substances and in which the investigation was made by experiments with animals even more insusceptible to this virus than were pigeons to the virus of hog cholera. Roux used guinea pigs, and he tells us that "cattle and sheep are the animals that most easily take the *Charbon Bactérien* the guinea pig shows more resistance to this disease; it does not always die as the result of inoculation made with the pulp of the charbonous tumor. MM. Arloing, Cornevin and Thomas have made known a mode of inoculation which always produces death to guinea pigs. To obtain this result it is sufficient to mix the virus with a solution containing twenty per cent of lactic acid. With this means of proof there need be no fear of a failure of the inoculations or of seeing the control guinea pigs resist them."

It is a rather peculiar idea to advance that we must receive without question experiments made in Pasteur's laboratory with animals so insusceptible that a foreign substance must be added to the virus in order to produce positive results, while exactly similar experiments must be rejected, when made in this country with virus, to which it is not necessary to add any foreign substance, and which, under the conditions mentioned, never failed to produce positive results. This objection was apparently disposed of by Hueppe, however, when he called attention to the control experiments and the positive results of these inoculations.

The question is, however, apparently deemed of too much importance to Pasteur's laboratory to be allowed to rest here. In Duclaux' review a new objection is relied upon to show that my experiments were valueless. It is that the liquids were sterilized at a temperature too near to the limit at which the microbe is destroyed. Duclaux claims that testing this liquid by adding a few drops to a fresh culture tube, in order to learn whether there are any living organisms present, proves nothing. "It was known," he goes on to say, "and it is still better known now, that microbes which are present in a culture are not identical, and that they have different powers of resistance to external agents. These powers of resistance are not identical, although approaching each other closely, and the more closely, the more homogeneous the culture. Nothing proved then and nothing proves yet, that all of the microbes were killed at this temperature limit of 58°–60°."

These objections were anticipated when our experiments were planned, and anyone who will go carefully into the details of these experiments will see that the objections do not hold good. It is true that the temperature of 58°–60°, at which we sterilized our culture liquids, is rather near to the limit which is necessary to destroy all life in these cultures. While admitting that there may be different degrees of resistance with this germ, however, we demonstrated that 58° maintained for ten minutes was sufficient to destroy those germs with the greatest powers of resistance, because none were left capable of multiplying after they had been so treated. In our experiments on immunity, we do not limit the cultures to a temperature of 58° for ten minutes; on the contrary, we increased the temperature one or two degrees and we maintained this increased temperature for two hours — in other words for twelve times as long as had been found necessary to destroy all living germs. It will also be seen by reference to the details of our experiments that, in some of these, the culture liquids employed were concentrated over a waterbath. In these experiments the culture liquids were heated to a temperature of nearly 100° for from one to two hours. This is far beyond the point where there could be any doubt of the germs being completely destroyed. While some of the experiments in which this evaporated virus was used were successful, we recognized the fact, apparently confirmed by the French investigations, that such a temperature weakened the power of the liquid for the production of immunity. We consider, therefore, that this objection of Roux is absolutely untenable and that there can be no question that the virus was completely sterilized in our experiments.

As another reason for his belief, Roux cites the small quantity of sterilized culture which was sufficient to produce immunity. "Thus," he says, "in the first of these experiments published in the Report of 1885, a dose of $\frac{5}{10}$ cc. of vaccinal culture sufficed to confer almost complete immunity on a pigeon. This dose differs greatly from that which is necessary to confer immunity in other diseases." Roux evidently errs in this statement. By referring to the Report of 1885 it will be found that one pigeon received $\frac{5}{10}$ cc., and when inoculated with strong virus died in forty-eight hours, *i. e.*, this bird had no immunity whatever. Another one received 1.5 cc., and when inoculated with active virus was so affected that it lost the use of its legs although it survived the inoculation. In this case there was evidently a partial immunity. All of the other birds referred to in that Report received over 3 cc. In our subsequent experiments we found that 2 cc. would generally confer immunity on pigeons, although in many experiments we used 3 to 4 cc.

When we turn to the experiments made in Pasteur's laboratory, we find that this quantity is not so different from what was used there as we would be led to believe from the remarks quoted. For instance, while in the experiments of Roux and Chamberland, for producing immunity in guinea pigs against the *Vibrion septique*, they used 120 cc. of culture liquid which had been heated to 105°–110°. They succeeded in producing equal immunity with doses of only 1 cc. of the serum from the muscles and cellular tissue, which was sterilized by filtration instead of by heat. It would appear

that the high temperature which they used for sterilizing their culture liquids destroyed to a certain extent the activity of the substance which produced immunity, just as in our experiments a still lower temperature had such an effect. If doses of 1 cc. of filtered serum repeated seven or eight times are sufficient to produce immunity in an animal of the size of a guinea pig, which weighs from 400 to 500 grammes, it is not surprising that an equal dose of sterilized culture liquid repeated twice should be sufficient to produce immunity in a pigeon which only weighs from 200 to 250 grammes. Roux also succeeded in producing immunity in guinea pigs against symptomatic charbon by doses of 1 cc. of filtered serum repeated from ten to twelve times. With these experiments before him we are surprised that Duclaux should raise this objection as to the size of the doses of sterilized culture used in our experiments.

The final objection raised by Duclaux is that this method of vaccination had failed with the more susceptible animal species; that it succeeded only with the pigeon, a species on the marginal line of immunity, and that this mode of vaccination succeeded better in winter than in summer, and that even in winter there are some failures. It is true that our experiments failed with large animals like pigs, which are more susceptible to the disease, but this has no bearing on the demonstration of the principle. With these large animals, and especially in this disease, there are other difficulties to be overcome. The large size of the animal requires a great increase in the amount of the material used, an amount for the determination of which, sufficient experiments have not been made. Again the manner in which the virus is introduced into the body under natural conditions has a very important influence. Indeed it is doubtful if the immunity of pigs can be artificially raised to such a degree as to prevent the multiplication of the germs on the mucous surface and within the walls of the intestines and the formation of those large ulcers which are characteristic of the malady and are usually fatal. It is not to be forgotten that M. Roux has not published any experiments in which he has succeeded by this method in conferring immunity on cattle or sheep against symptomatic anthrax; but I have not heard of this being urged as an objection to the conclusions from the experiments on immunity with guinea pigs which he has published.

Duclaux is wrong in supposing that our experiments only succeeded with pigeons in winter. We did not make this statement. Pigeons are more susceptible to the virus of hog cholera in winter than in summer; consequently we selected winter for the experiments because at that time our control experiments with unprotected birds were successful. It is true that some of our experiments failed, but I imagine no principle has been worked out experimentally in which all the experiments from the beginning were successful. It was necessary for us to determine experimentally the dose which was to be given to produce immunity and the number of times it was necessary to repeat the dose. We also endeavored to learn the effect of a boiling temperature on the soluble substances produced in the cultures of this germ, and the high degree of heat is re-

sponsible for the failure of some of the experiments. It also required some experimenting to learn that pigeons are more susceptible in winter that in summer. We published all of our experiments. M. Roux avoids the charge of failure by publishing only successful experiments. Taking all these facts into consideration, it must be admitted that our experiments published in 1886 were successful, and that they demonstrated the production of immunity by soluble substances contained in the culture liquids in which the germ of hog cholera had multiplied. I therefore continue to claim priority for this important discovery.

SOME NEW EXPERIMENTS IN PRODUCING ANÆSTHESIA WITH NITROUS OXIDE AND AIR, AND NITROUS OXIDE AND OXYGEN IN CONDENSED AIR-CHAMBERS, ILLUSTRATED BY EXPERIMENTS ON AN ANIMAL. By Dr. E. P. HOWLAND,[1] of Washington.

THERE are forty-seven substances mentioned and described as anæsthetics in the latest works on anæsthesia. Four of them, nitrous oxide, nitrous oxide in mechanical combination with oxygen, ether and chloroform, are the anæsthetics that have received the sanction of the medical and dental professions, as the best and safest means of producing insensibility to pain during surgical and dental operations.

The cause of anæsthesia and the action of anæsthetics are still subjects of dispute between those who have had the largest experience in their administration, and who have made the most critical experiments on animals and man; but the best methods of administering and the effects of anæsthetics on the mental organism are subjects that can be demonstrated by experiment and practical experience.

Nitrous oxide has now been administered to millions of persons, and it has been proved to be almost absolutely safe as an anæsthetic for short operations, but has proved to be dangerous and unpractical for prolonged operations on account of its effects and the want of acquired skill and experience in its administration.

Dr. Goodwillie of New York,and myself,and a few others have been successful in its administration for surgical operations; but the successful administrators in the United States can be numbered on your fingers. Dr. J. Marion Sims in a letter to Johnston Bros., dated Jan. 25, 1872, says, "Since last September I have performed a great many operations on patients under the influence of nitrous oxide. Many of these took the gas for twenty, twenty-five, thirty and thirty-five minutes. One took it for fifty minutes, and I saw no reason why she could not have safely taken it twice that length of time. Dr. Goodwillie has given the gas in two ovar-

[1] Dr. Howland died suddenly on his way home from the Cleveland meeting.—EDITOR.

iotomy cases for me; one for twenty-seven minutes, and the other for thirty-one minutes. In these it was all that I could wish."

I have administered nitrous oxide in many thousand dental operations, and several hundred surgical operations in the city of Washington, the longest time being thirty-five minutes for a capital operation performed by Dr. Bliss, assisted by Dr. Stuart. If nitrous oxide had been proved a safe and practical anæsthetic for prolonged operations, ere this it would have been in universal use, and would have taken the place of all other anæsthetics. The reason why it is not safe, is, because the blood is not oxygenated during its administration, and the patient will die of asphyxia in two and half minutes if compelled to breathe pure nitrous oxide.

If nitrous oxide is mixed with five per cent or more of oxygen, or atmospheric air, the gas is so diluted that it will not produce insensibility, and it takes a much larger quantity of oxygen or air than five per cent to prevent its being fatal to the patient from asphyxia. Prolonged anæsthesia is produced by breathing pure nitrous oxide from one-half to three-fourths of a minute till the patient is anæsthetized, and then breathing air from one-fourth to one-half a minute to partially oxygenate the blood and then breathing nitrous oxide again before the return of consciousness; recovery or death of the patient occurring within the limits of one and a half minutes. In my experiments on warm-blooded animals, death has generally occurred within two and a half minutes, the animal in a few cases living four minutes.

The non-oxydation of the blood during the breathing of nitrous oxide is proved by the livid appearance of the face of the patient, the examination of the blood of animals after death, and the chemical analysis of the expirations of persons under its influence.

From a large number of experiments, I have found that all of the nitrous oxide taken into the lungs during three-fourths of a minute of inhalation is not exhaled in this time, but as much is exhaled as of pure hydrogen or nitrogen when they are breathed for three-fourths of a minute, and no more carbonic acid is found in the exhaled nitrous oxide than in the exhaled hydrogen or nitrogen, proving conclusively that the blood is not oxygenated from the decomposition of the nitrous oxide in the circulation. Some of our most eminent men and practical chemists and physiologists are in error in relation to the decomposition of anæsthetics in the circulation and their elements forming new compounds.

We have a new anæsthetic that should claim our earnest attention. Nitrous oxide and oxygen mixed in the proportion of thirty-five parts of nitrous oxide and fifteen parts of oxygen, and administered in a condensed air chamber under a pressure of five pounds per square inch, will produce perfect anæsthesia for any length of time required, and the blood at the same time being perfectly oxygenated by the free and uncombined oxygen there is no danger or injury to the patient from a partial or entire asphyxia, as there is from nitrous oxide alone, or the still greater danger and injury from ether and chloroform.

In my experiments on animals with a mixture of equal quantities of nitrous oxide and atmospheric air, in comparison with a mixture of eighty-

five parts of nitrous oxide and fifteen parts of oxygen, I find that they are equal in their anæsthetic effects; but the pressure in the condensing chamber has to be fifteen pounds to the square inch with nitrous oxide and air, and only five pounds pressure to the square inch with nitrous oxide and oxygen.

From the thousands of operations performed in France under the influence of nitrous oxide and oxygen in condensed air-chambers, and from my own experiment on animals similarly anæsthetized, I believe that nitrous oxide and oxygen will be *the* anæsthetic of the future, and I think that greater facilities in administering this agent, regulating the pressure and purifying the air from infectious germs in a chamber that I have devised, will greatly facilitate its introduction.

This anæsthetic is going to be of great service to the dentist as well as the surgeon. By increasing the pressure of air in the chamber, a large quantity of gas is administered, and the patient remains a much greater length of time anæsthetized after ceasing inhalation of nitrous oxide and oxygen, than with pure nitrous oxide as ordinarily given, thereby enabling the dentist to extract a large number of teeth before recovery. Refractory patients and those that do not readily become anæsthetized under pure nitrous oxide, become perfectly passive and quickly insensible under nitrous oxide and oxygen. The operator is not at all affected by the escape of nitrous oxide in the chamber, and he experiences no unpleasantness or injury as is often the case with the administration of ether and chloroform. The dangerous point of all anæsthetics is when the toxic effect of the substance taken into the system, paralyzes the nerve centres, and they are on the point of ceasing to control and direct vital action, or when the substance as a vapor or gas is taken into the lungs in a quantity that excludes the required amount of oxygen to arterialize the blood, and death may occur from asphyxia. The toxic point of danger from nitrous oxide, I have proved by experiments on animals to be beyond five hours of continued anæsthesia when the blood is perfectly oxygenated with free oxygen, and the danger point from asphyxia is when the volume of oxygen with the nitrous oxide is below ten per cent of the mixture.

The danger point is therefore entirely under the control of the administrator.

If a greater amount of oxygen than fifteen parts to eighty-five of nitrous oxide is administered, perfect anæsthesia is not produced unless the pressure of the chamber is increased just in proportion to the amount of oxygen added. When the oxygen is added it makes the whole quantity equal to the nitrous oxide, the pressure of the chamber has to be fifteen pounds to the square inch, the same as with equal quantities of nitrous oxide and air.

Prolonged administration of this mixture is injurious, the same as prolonged administration of pure oxygen, causing super-oxidation of the blood and inflammatory action.

I have kept animals perfectly anæsthetized for two hours in a mixture of eighty parts of nitrous oxide and twenty of oxygen under a pressure of ten pounds to the square inch without any apparent injury. The time

of the dangerous toxic point of anæsthesia, and also the time of the danger point from asphyxia in all of my experiments with ether and chloroform have proved to be uncertain and unreliable, and the danger point to be beyond the control of the operator, death often occurring without apparent cause. Many professional men declare it criminal to use chloroform instead of ether as there are ten deaths from the former to one from the latter; while to those who survive the effects of these anæsthetics, positive injury results from chloroform in still greater proportion.

From statistics by Professor E. Andrews in the Chicago Medical Examiner, the death rate from the administration of chloroform is one in 2,723, from sulphuric ether, one in 23,404, from mixed ether and chloroform, one in 5,588, while during the year 1887, in the United States and Europe, nitrous oxide was administered to over one million persons and not a single death occurred from its use.

Is it not the duty, therefore, of every professional man to lend his influence to bring into use in every city and hospital an anæsthetic that is a hundred thousand times safer than are ether and chloroform?

I am happy to say that in my endeavors to introduce the new anæsthetic, I am meeting the almost universal approbation of the medical and dental professions.

Experiment. — An animal was placed in an anæsthetic chamber under pressure, and remained perfectly anæsthetized while the pressure was at five pounds to the square inch, but when reduced below this pressure showed signs of recovery. When the pressure was raised again, the animal became quiet; when taken from the chamber, the animal was insensible to pain but soon recovered as well as ever.

A PHASE OF EVOLUTION. By DR. E. LEWIS STURTEVANT, South Framingham, Mass.

[ABSTRACT.]

BOTANICAL varieties, the unit of evolutionary progress under vegetable culture, as evidenced by a study of the cultivated and wild dandelion. Drawings (colored) and herbarium specimens exhibited, demonstrating the substantial identity of garden varieties with wild varieties, and conversely showing the types for new varieties of the future.

[Published in full in Proceedings of the Society for the Promotion of Agricultural Science, 1888, p. 77.]

A STUDY OF THE HYDRANGEA, AS TO THE OBJECTS OF CROSS-FERTILIZATION. By Prof. THOMAS MEEHAN, Germantown, Philadelphia, Pa.

[ABSTRACT.]

THE author details a number of facts connected with the structure and development of allied species of Hydrangea, and argues that on no theory

of evolution based on adaptation to insect visits, could these changes occur. He contends that variety must exist, in order to provide for order,—and that variations as we see in Hydrangea can be for no special benefit to the plant but for mere variety's sake.

SOME NEW FACTS IN THE LIFE-HISTORY OF YUCCA AND THE YUCCA MOTH By Prof. THOMAS MEEHAN, Germantown, Philadelphia, Pa.

[ABSTRACT.]

THE author gives some figures in regard to the time of opening, and the duration of the flowers,—with notes on the time and duration of the moisture exudation from the perianth. The cause of the sudden stoppage of the waste is discussed, and some remarkable facts in connection with the behavior of the insect *Pronuba yuccasella*, observed by Professor Riley, confirmed.

The remarkable adaptation by which an insect is made to do the work of self fertilization, which the plant could just as readily do itself,is contrasted with similar observations in the animal kingdom.

ON THE CAUSE AND SIGNIFICANCE OF DICHOGAMY IN FLOWERS. By Prof. THOMAS MEEHAN, Germantown, Philadelphia, Pa.

[ABSTRACT.]

THE author repeats the announcement of his discovery, already made through the Proceedings of the Academy of Natural Sciences of Philadelphia, that it takes a longer-continued amount of heat to excite growth in the female than the male organs of flowers; and that varying seasons will, therefore, advance or retard the several sexual organs accordingly. So there is absolutely nothing in connection with the visits of insects to account for dichogamy, which is solely an accident of climatic environment.

ADAPTATION IN THE HONEYSUCKLE, AND INSECT VISITORS. By Prof. THOMAS MEEHAN, Germantown, Philadelphia, Pa.

[ABSTRACT.]

THE author gives all the points in the flowers of certain honeysuckles, and the development of the extraordinary amount of nectar from which the family derives its common name, and shows that the insects which visit the flower for the sweet secretions take no part in the pollination of the flower, which office is performed by pollen-gathering insects for which no special adaptation has been made, and which rather aid self- than cross-fertilization.

NOTES ON THE INFLORESCENCE OF CALLITRICHE. By Prof. JOSEPH SCHRENK, New York College of Pharmacy (Hoboken, N. J.).

[ABSTRACT.]

THE inflorescence briefly described. The bracts, considered as stipules by Caspary and as trichomes by Schenck, are really sacs or bladders filled with air. Their appearance, structure and development described. Their function is to increase the buoyancy of the tufts of leaves with the pistils and stamens in their axils. The palm-shaped hairs at the nodes are probably secretory organs. The histology of the fibro-vascular bundles shows that when pistil and stamens occur in the same axil, they are to be considered as a perfect flower.

HYGROSCOPIC MOVEMENTS IN THE CONE-SCALES OF ABIETINEÆ. By Prof. ALBERT N. PRENTISS, Ithaca, N. Y.

[ABSTRACT.]

IN most of the Abietineæ, soon after the maturation of the cones, the persistent scales fold backward, or outward from the axis, to permit the ripened seeds to escape. The scales are very sensitive to moisture, and in many species exhibit very rapid movements when wet, as with rain. This is especially well seen in the cones of *Tsuga Canadensis*, in which the widely-open scales become completely closed in twelve minutes.

This property of the cone scales, is found to be very efficient; first in loosening the seeds with their attached wing from the scale which bears them; and second, in favoring the wide dispersion of the seeds, as the cones open and close many times before all the seeds are sown, thus securing their transport in different directions by the varying winds.

SEXUAL CHARACTERS OF THE SPECIES OF THE COLEOPTEROUS GENUS LACHNOSTERNA. By JOHN B. SMITH, U. S. National Museum, Washington, D. C.

[ABSTRACT.]

THE species of *Lachnosterna*, though large in size, have always been most difficult to separate. The most recent work of Dr. George H. Horn on this genus has aided in the separation of the species, while not clearing up doubts in all the cases. In examining a very large series of freshly captured specimens, differences in sexual structure of the males became apparent, which cleared up all doubts of the specific limitation of species, and examination of a series of females confirmed the conclusions drawn from the males. Heretofore determination of the ♀ has been difficult

without having also the ♂ at hand. In entomological literature thus far there has been considerable attention paid to these structures of the male, while I cannot find that the genitalia of the females have been examined. As a matter of fact they are correlated to those of the ♂, and every modification of the male is accompanied by a corresponding difference in the female. The further fact developed in the course of investigation, that there was absolutely no variation in these structures in either sex, within the limits of a species. The conclusions are therefore that however variable in superficial characters of color, vestiture, within some limits of sculpture as well, identity of genital structure indicates identity of species; while however similar in superficial characters, difference in genitalia indicates difference of species.

On the structure of the skull of the larva of Amphiuma. By Prof. O. P. Hay, Irvington, Ind.

[ABSTRACT.]

This paper described the structure of the skull of the larva of *Amphiuma* while still within the egg, but near the time of hatching. The examination showed that its development is much further advanced than is that of related forms when they are excluded. There is a large fontanelle at the base of the skull on each side of the cranial notochord. The trabeculæ are slender, enclosing a large fontanelle and meeting in the nasal region. The cranial walls are low and the brain is mostly protected above by membrane alone. The otic capsules are large and have coalesced with the trabeculæ. The condyles are prominent and are ossified. The suspensorium has three processes: pedicle, otic process and an ascending process. Articulated to its posterior edge is a rod of cartilage, which overlies the facial nerve.

The hyoid is partially ossified, as is also a portion of the first branchial arch.

The following membrane bones are developed: premaxillaries, vomers, frontals, parietals, squamosals, dentaries, angulo-splenials. No maxillaries are seen, but a row of dental papillæ shows where they will shortly appear.

Notice of a supposed new species of Branchipus from Indiana. By Prof. O. P. Hay, Irvington, Ind.

[ABSTRACT.]

Most closely related to *B. bundyi* from Jefferson, Wisconsin. Differs therefrom in that the female has growing out from each dorso-lateral surface of the tenth segment a prominent lobe or process. Found at Irvington, Indiana. Named *B. Gelidus*, W. P. Hay.

COLOR VARIATIONS OF NEBRASKA FLYING SQUIRRELS. By Prof. W. EDGAR TAYLOR, State Normal School, Peru, Neb.

[ABSTRACT.]

THE American flying squirrels present a range of geographical variation in size quite unparalleled in other members of the squirrel family. On the other hand the coloration is remarkably constant, almost exceptionally so; specimens from the same locality sometimes differ in the color of the dorsal surface as much as the most diverse examples from widely separated localities. Two varieties are recognized by naturalists, probably grading into each other. From a careful examination of typical specimens of the flying squirrel from Nebraska we may conclude:

1. The measurements correspond to the northern variety, while the colors correspond more nearly with the southern variety.
2. The local variations, in colors, are very great; these variations existing even in members of the same nest.
3. Locally, at least, the two varieties appear to grade into each other.
4. The degree of coloration on the same parts of different specimens does not vary in the same ratio.

THE MUSCLES OF THE SOFT PALATE IN THE DOMESTIC CAT. By Prof. T. B. STOWELL, Cortland, N. Y.

[ABSTRACT.]

THIS contribution to comparative anatomy is preliminary to an inquiry into the phylogenesis of the *uvula palati*. Two preparations are necessary for the study of the muscles of the soft palate; a hemisected and a transected head. In the *hemisection* can be seen to best advantage the relations of the respiratory and the alimentary passages, the eustachian tube, the tonsil and the epiglottis. The following muscles should be dissected in the same section: the levator, the pharyngo-staphyle and the glosso-staphyle. In the *transection* should be studied the length of the palate and its function in closing the passage from the oral to the pharyngeal cavity, thereby insuring an exclusively nasal respiration; in this section are demonstrated the origins of the levator and the tensor muscles.

The soft palate is a continuation of the hard palate without the osseous plate. The muscles are striated, paired, receive their blood-supply from the ectal carotid artery and are innerved by the trigeminus, the facial and the glosso-pharyngeal nerves. The staphyle muscles shorten the velum and close the postnares. The levators act upon the middle of the velum and are most active in closing the postnares. The dilation of the ventral opening of the eustachian tube, usually referred to the tensor muscle, may be due to the action of the levators in closing the postnares so firmly that the forcible introduction of air expands the tube and drives the tympanic membrane laterad.

The tensor gives firmness to the palate especially in deglutition. The pharyngo-staphyle and glosso-staphyle muscles close the *isthmus of the fauces* in deglutition.

COMPARISON OF THE FLORA OF EASTERN AND WESTERN MICHIGAN IN THE LATITUDE OF 44° 40[1]. By Prof. W. J. BEAL, Agricultural College, Mich.

[ABSTRACT.[1]]

ON the western shore next to Lake Michigan, we find many southern plants not found on the eastern; on eastern many northern plants not found on western. Lists were given.

OBSERVATIONS ON THE SUCCESSION OF FORESTS IN NORTHERN MICHIGAN. By Prof. W. J. BEAL, Agricultural College, Mich.

[ABSTRACT.[1]]

A STUDY of virgin forests shows very old, stunted plants like those in cleared land near by, excepting some coniferæ which are killed by fire. Fire has more than once been through some of the forests studied, as shown by the former killing of small trees to the ground. *Pinus Banksiana* is admirably adapted to succeed itself, as it fruits young, fruits abundantly, seeds retain vitality in cones closed up often for seven years or more. Some cones do not open until trees are killed by fire, then seeds drop on ground ready to favor growth.

THE SYSTEMATIC POSITION OF THE RHIZOCARPEÆ. By DOUGLAS H. CAMPBELL, Detroit, Mich.

[ABSTRACT.]

A. THE present position of the *Rhizocarpeæ*.

B. Outline of the work of different investigators on the Rhizocarpeæ.

C. Review of the author's work on various members of the order, and comparison with other Pteridophytes.

D. Conclusions.

POLLEN GERMINATION AND POLLEN MEASUREMENTS. By Dr. BYRON D. HALSTED, Agricultural College, Iowa.

[ABSTRACT.]

1st part—(1) The old methods of germination. (2) Tests on glass slides—in watch-glasses—in artists'well-slabs. (3) The control the method offers. (4) Facilities for comparative study of pollen.

2nd part—Measurements. (1) When dry and what is shown. (2) When wet and points demonstrated. (3) Importance of measurements being taken for both wet and dry pollen.

[1] Printed in full in the Report of the Michigan State Board of Agriculture for 1888.

OBSERVATIONS ON THE HOUSEBUILDING HABIT OF THE MUSKRAT. By AMOS W. BUTLER, Brookville, Ind.

[ABSTRACT.]

THIS paper gave some observations on the structure of muskrat homes, noting certain changes that have occurred in the habits of the animal within historic time. The author also referred to the extension of the housebuilding habit and its rate of advancement, devoting attention to the causes of the changes noted. The observations were all made within the southeast quarter of Indiana.

METAMORPHOSIS IN THE PLEURUM OF ALEURODES. By Prof. HERBERT OSBORN, Ames, Iowa.

[ABSTRACT.]

OBSERVATIONS on a species of *Aleurodes* during prepupal, pupal, and the imago stages show that the pleurum in prepupal stage is equal in extent, and not separable from the scales; that during the pupal stage it is detached from scale and in meso-and meta-thorax is rapidly altered in structure, while in prothorax and abdominal segments lobes are clearly defined. In the fresh imago slight pressure from above causes the extrusion of these lobes on prothorax and abdomen, showing that the pleurum is simply contracted to form the walls of the body, while no such expansion in meso- and meta-thorax indicates the modification of this portion of the pleurum to form the wings. Specimens showing these different steps in development were submitted for examination.

ENTOMOLOGICAL MEMORANDA. By Prof. O. S. WESTCOTT, Maywood, Ill.

[ABSTRACT.]

(1) Further evidence of the carrion-loving propensities of certain diurnal Lepidoptera.

(2) Alleged seasonal varieties of *Melitæa tharos*, viz., *marcia* and *Morpheus* equally abundant in the latter part of July at Port Arthur, Ontario, Canada.

(3) Pupation of nocturnal larvæ on a gravel walk.

(4) Evidence of the greater abundance of *Lachnosterna gibbosa* Burm. than that of *Lachnosterna fusca* Fröhl, in some localities.

(5) Vast numbers of *Crambidæ* taken at light.

(6) A successful insect trap.

A PLEA FOR UNIFORMITY IN BIOLOGICAL NOMENCLATURE. By Dr. N. L. BRITTON, Columbia College, New York.

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ADDRESS

BY

DR. CHARLES C. ABBOTT,

VICE-PRESIDENT, SECTION H.

EVIDENCES OF THE ANTIQUITY OF MAN IN EASTERN NORTH AMERICA.

An ecclesiastical body has recently decided, by a significant vote, that man, perfect in all his parts, had been created *de novo* from the dust — that the law of evolution has nothing whatever to do with him or his; if, indeed, it is not a very flimsy figment of the imagination, and a harmful plaything with which men who aim to be scientific, or rational, solace themselves, because, in their foolhardiness, they decline to accept the asserted initial separate creation of all living beings, from the highest to the lowest, now living or that have lived.

If this decision of an ecclesiastical body really represented the truth, instead of being a painful exhibition of stultifying ignorance, Anthropology would be shorn of much of its attractiveness, and the term "prehistoric" would have little, if any, meaning. In such a case, the races of America would scarcely be worthy of consideration, being but the rapidly degenerated and discolored descendants of the physically perfect, yet painfully weak-kneed Adam of Paradise.

There is, on the contrary, unquestionably but one method of correctly interpreting the past, as to the history of man, and that is to adopt the same methods and draw the same inferences as have been done in tracing the evolution of the horse, camel, elephant or ox. This, strangely enough, seems repugnant to very many who feel that any relationship, however remote, with less intelligent creatures is a reflection upon their own intelligence; while, in fact,

they compromise their claim to a high intellectual standard only when they deny their purely animal origin.

To determine at what precise point in geological time, man appeared upon the earth, is, it seems to me, obviously impracticable, from the fact that the dividing line separating humanity from the non-human cannot be drawn. It were as easy to name the moment when the gloaming merges into night, or shout with confidence, now! as the dawn brightens into day. Nor is it demonstrable, with our present knowledge, to point to that country where the momentous change first took place, if it occurred but once. At present, however, we can safely say that Miocene man is extremely problematical, and Pliocene man a question as yet unsettled; the auriferous gravels of California being pronounced late Tertiary by Whitney, and by LeConte as representing "the beginning of the Glacial Epoch."

At all events, we have neolithic man as far back as the Glacial Epoch and possibly in the Pliocene. Man in the Tertiaries, therefore, championed by my honored predecessor, Professor Morse, becomes something more tangible than a hypothetical creature. Professor Putnam has pithily outlined this important subject in a recent communication to the Boston Society of Natural History. He there remarks: "When we compare the facts now known from the eastern side of the continent, with those of the western side, they seem to force upon us to accept a far longer occupation by man of the western coast than of the eastern; for not only on the western side of the continent have his remains been found in geological beds unquestionably earlier than the gravels of the Mississippi, Ohio and Delaware valleys, but he had at that early time reached a degree of development equal to that of the inhabitants of California at the time of European contact, so far as the character of the stone mortars, chipped and polished stone implements, and shell beads, found in the auriferous gravels, can tell the story. On the Pacific coast, where the conditions of life were more favorable, he had passed beyond the palæolithic stage before his works were buried in the gravels under the beds of lava; while at a later period on the Atlantic coast he was still in the palæolithic stage. Either this must be accepted, or else the geological changes on the Pacific coast have been entirely misunderstood; for we can no longer question the many instances of the discovery of the works of man, and also of his bones, in the Californian gravels. The

same story is told by the beautifully chipped implement of obsidian found by Mr. McGee in the quaternary deposits of Lake Lahontan in Nevada."

Man in America, therefore, must be studied from a geological standpoint; and not only, as we have seen, is this true of the Pacific coast, but signally so, when, coming eastward, we reach the Mississippi.

Mr. Warren Upham has, during the present year, published in full, a lucid account of his careful examinations of the drift formations at, and in the vicinity of Little Falls, Minnesota, where, in 1879, Miss Babbitt found those extremely rude but unquestionably worked quartzes, concerning which there has been much needless comment, unfavorable to their human origin or their asserted antiquity, even from presumably learned sources; and, of course, lame attempts to belittle the discovery by those who should know better are still heard.

The conclusion of Mr. Upham's paper is as follows: "While the deposition of the valley-drift at Little Falls was still going forward, men resorted there, and left, as the remnants of their manufacture of stone implements, multitudes of quartz fragments. By the continued deposition of the modified drift, lifting the river upon the surface of its glacial flood-plain, these quartz chips were deeply buried in that formation. The date of this valley-drift must be that of the retreat of the ice of the last glacial epoch, from whose melting were supplied both this sediment and the floods by which it was brought. The glacial flood-plain, beneath whose surface the quartz fragments occur, was deposited in the same manner as additions are now made to the surface of the bottom-land; and the flooded condition of the river, by which this was done, was doubtless maintained through all the warm portion of the year, while the ice-sheet was being melted away upon the region of its head-waters. But in spring, autumn and winter, or, in exceptional years, through much of the summer, it seems probable that the river was confined to a channel, being of insufficient volume to cover its flood-plain. At such time this plain was the site of human habitations and industry."

In 1883, as the result of exhaustive studies of glacial deposits, from New Jersey westward, across Ohio, Rev. G. Frederick Wright predicted that traces of palæolithic man would be found in the latter state. Commenting upon such evidences as occurring else-

where, he remarks: "Man was on this continent at that period when the climate and ice of Greenland extended to the mouth of New York Harbor. The probability is that if he was in New Jersey at that time he was also upon the banks of the Ohio, and the extensive terrace and gravel deposits in the southern part of our state should be closely scanned by archæologists. When observers become familiar with the rude form of these palæolithic implements they will doubtless find them in abundance."

Palæolithic implements, concerning which there can be no doubt, have not been discovered in abundance as yet, but Professor Wright's belief proves to have been well founded. Dr. C. L. Metz of Madisonville, Ohio, has discovered two specimens which set the matter at rest. Both were found at significant depths; one of them, nearly thirty feet below the surface. The region, where found, is one characterized by immense gravel deposits of glacial age and origin. They have been carefully studied and reported upon by Prof. Putnam and by Mr. Wright, who remarks, in conclusion, with reference to the discoveries of Dr. Metz:

"In the light of the exposition just given, these implements will at once be recognized as among the most important archæological discoveries yet made in America, ranking on a par with those of Dr. Abbott, at Trenton, N. J. They show that in Ohio, as well as on the Atlantic coast, man was an inhabitant before the close of the glacial period. We can henceforth speak with confidence of interglacial man in Ohio. It is facts like these which give archæological significance to the present fruitful inquiries concerning the date of the glacial epoch in North America. When the age of the mound-builders of Ohio is reckoned by centuries, that of the glacial man who chipped these palæolithic implements must be reckoned by thousands of years."

Mr. Hilborne T. Cresson will, at this meeting, present notices of his discovery of two chipped implements of argillite which he found *in situ*, at a depth of several feet from the surface, in railroad cuttings through the old terrace of the Delaware river in Claymont county, Delaware. The geological position of these specimens will excite discussion, but their great age will not be questioned. Of particular interest in relation to discoveries in the gravels at Trenton and Ohio, is the discovery of a large flint implement found by Mr. Cresson in the glacial gravel in Jackson county, Indiana. An account of this specimen will also be presented to this section dur-

ing the meeting by Professor Putnam for Mr. Cresson. I am permitted to call attention in advance to these new facts in the distribution of palæolithic implements, and I may add that it is of extreme importance that these rude implements from New Jersey, Delaware, Ohio, Indiana and Minnesota are in the Peabody Museum at Cambridge, where they can be freely studied and compared with each other and with the specimens from the gravels of the old world.

Nor are these instances of the discovery of palæolithic man, in North America, all that are upon record; but are they not enough? Why, indeed, should the bare mention of the poor fellow's name still excite a sneer? There will probably always be over-cautious folk who will only accept *cum grano salis*, the Man of the Tertiaries, however eloquently he may be plead for; but no one willing to accept other testimony than his or her own eyes—often the most treacherous of guides—can in fairness turn their backs, when we speak of that primitive chipper of flinty rock, who, with no other wêapon, at least, held at bay, the savage beasts of primeval times; and who, with a cunning that is ever better than mere strength of limb, proved a powerful foe of both the mammoth and the mastodon. Such a man stands out in the geological history of the Mississippi and Ohio valleys, not as a dim shadow, but a substantial fact.

Was he confined to these two portions of the country? By no means. On the contrary, it would appear that as either seaboard was neared, his numbers increased, and that as a coast-dweller he preëminently flourished. In the valley of the Delaware river palæolithic man has left such abundant traces of his former presence, in the form of rudely-fashioned stone implements, that for long they were considered as the hasty or unfinished work of the later Indians.

As the first to point out what is now maintained by competent archæologists to be their real significance, I may be pardoned for devoting the conclusion of my address to a consideration of that region—the Delaware valley—so far as its physical character and the traces of prehistoric man found there have a bearing on the question of the antiquity of Man in America.

The literature of the subject is now so considerable—not including the inanities of the ignorant—that a brief résumé would of itself outreach reasonable limits, and I purpose therefore to confine myself more particularly to the results of my own work. But do

not suppose that others have not carefully gone over the same ground. Shaler, Belt, Whitney, Wright, Pumpelly, McGee, Carvill Lewis and our State Geologist, Cook, as geologists are practically one in their view that the gravel deposits are so far ancient as to be very significant as to whatever traces of man or other mammals, they may contain; while Dawkins, Tylor, Putnam, Morse, Haynes, Wilson and De Costa have all been more or less successful in finding traces of palæolithic man in this river valley, and admit without qualification, his former presence.

The question may now be asked, what is a palæolithic implement? It is not very readily defined as there is considerable variation in the shape; but as I understand the significance of the term, it is properly applied to coarsely chipped masses of flinty rock upon which a distinctly designed cutting edge is formed, to which is often added an acute point. Furthermore, they show unmistakable evidence of antiquity by the weathering of their surfaces, and they are found as a rule, but not necessarily always, in deposits of glacial or river drift with which they agree in age.

How far do these Trentonian Implements meet with these requirements? As their discoverer, I prefer to give the opinions of others, rather than my own. This is what Dr. M. E. Wadsworth, the lithologist, has said of them: "Certain of these specimens were placed in my hands in 1876 for examination, their lithological character then being unknown. They were found by macroscopic and microscopic examination to have been made from argillite, greatly indurated, and breaking with a conchoidal fracture. The specimens were weathered to a greater or less extent and showed plainly that the fractures must have been made long ago. A few small fractures of secondary character occur. This secondary chipping evidently took place long after the original fracturing, but also long ago, as is shown by the weathering of the surfaces of both the primary and secondary fractures. The few secondary fractures are probably natural, and could easily occur if subjected to the action Dr. Abbott supposes. The original chipping could not have taken place by any known natural causes acting upon rocks, so far as the writer has any knowledge. Of course it then brings us to the only agency that could do the work—man. The characters of the specimens, petrographically, bore out the statements made to me by Mr. Putnam, of the conditions under which they were found, whether upon the surface or in the gravels. I do

not see how it is possible that such correspondence of characters could exist unless the specimens were found under the conditions reported.

The lithological characters then show that the specimens are not natural forms; that being composed of a slow weathering rock, they must have been made long years ago; that many years later they were subject to other conditions, probably natural, by which part have been modified; that since then, they have lain for many, many years exposed to weathering agencies; some showing that they have been subject to this action while lying on or near the surface, and others while buried to some depth.

Their weathering corresponds to that observed on pebbles of similar composition in gravels elsewhere. It is to be remembered that all the weathering has taken place since the Abbott specimens were originally chipped.

The term weathering, as here employed, means the alteration and decay that have taken place on the *surface* of the *specimen*, but does not imply that it has been exposed on the *surface* of the *ground;* it may or may not have been; the weathering itself shows with greater or less clearness whether this occurred from surface exposure or not.

Part of the specimens shown me bore evidence that they had originally been exposed to weathering on the surface of the ground and been covered since, but the covering evidently took place ages ago, if the weathering that they have been subjected to since is any criterion.

The term "argillite," as employed by me, is used to designate all argillaceous rocks, in which the argillaceous material is the predominant characteristic; slate or clay-slate, clay-stone, etc., are simply varieties of it, the term slate being only rightfully used when slaty cleavage is developed. The argillite out of which these specimens were made has no trace of cleavage."

According to Professor Haynes, a skilled observer, who has given much time to the study of palæolithic man in Europe, as well as in this country, "the term palæolithic is primarily restricted in meaning to such objects . . . . when met with under peculiar geological conditions; that is to say, when found embedded in the gravels which have been deposited by certain rivers during the period known to the geologists as the quaternary or pleistocene period. At that time their volume of water was much greater

than it now is, which was caused by the melting of the great ice-cap that once covered the northern portion of both continents, accompanied by a climate much more humid than we have at present. Such accumulations of gravel are often of very great thickness and embedded in them, side by side with the stone implements above described, are found the fossil bones of extinct species of animals, such as the mammoth." After mentioning, in the same paper, the various localities in Europe that he had carefully examined, he remarks with reference to the locality under consideration:

"From these various experiences I feel myself warranted in stating that the general appearance of the country and the character of the gravels at Trenton, N. J., present a most striking resemblance to what I have seen in the various localities in the Old World to which I have referred. There is the same rudely stratified mingling of coarse materials marked by a similar absence of clay . . . . Speaking . . . from an archæological standpoint, I do not hesitate to declare my firm conviction that the rude argillite objects found in the gravels of the Delaware river, at Trenton, N. J., are true palæolithic implements."

My own impressions of their true character was not suddenly reached. The evidence, of other kind, of the antiquity of the Indian, led me to consider them as rude objects made for some trivial purpose and discarded. Later, I became convinced that they were older than ordinary surface-found relics, and assumed that the Indian of history commenced his career in this valley while in the palæolithic stage of culture.

Thus, while pursuing my collecting of Indian relics, it was gradually forced upon my mind that these rude implements were more intimately associated with the gravel than with the surface of the ground and the relics of the Indians found upon it.

Acting upon this, I continued for two years to examine most carefully both the surface of our fields and every exposure of the underlying gravels; and in June, 1876, after having found several chipped implements *in situ*, expressed the opinion that the Delaware river, "now occupying a comparatively small and shallow channel, once flowed at an elevation of nearly fifty feet above its present level; and it was when such a mighty stream as this, that man first gazed upon its waters and lost those rude weapons in its swift current, that now, in the beds of gravel which its floods

have deposited, are alike the puzzle and delight of the archæologist. Had these first comers, like the troglodytes of France, convenient caves to shelter them, doubtless we should have their better wrought implements of bone to tell more surely the story of their ancient sojourn here; but wanting them, their history is not altogether lost, and in the rude weapons, now deeply embedded in the river's banks, we learn, at least, the fact of the presence, in the distant past, of an earlier people than the Indian."

Thus it will be seen that I have been fairly cautious in my statements and slow in reaching any conclusions with reference to these implements which separated them from ordinary Indian relics.

In September, 1876, Mr. Putnam, the Curator of the Peabody Museum of Archæology at Cambridge, Mass., favored me with a visit, and together we carefully examined the river bluff below Trenton, and succeeded in finding *two* specimens *in situ*, such as I had previously described in the American Naturalist. At his request, I continued my examinations of these gravels, acting under an appropriation made by the Peabody Museum for this purpose; and, in November of the same year, submitted to him a report *On the Discovery of Supposed Palæolithic Implements from the Glacial Drift in the Valley of the Delaware River, near Trenton, New Jersey*. Still realizing how all-important it was in this matter to make haste slowly, I purposely referred to these chipped stones as *supposed* palæolithic implements, and gave, in detail, my reasons for thus considering them.

Referring to this report, Mr. Putnam remarked, in his annual report to the trustees of the Peabody Museum, that "from a visit to the locality with Dr. Abbott, I see no reason to doubt the general conclusion he has reached in regard to the existence of man in glacial times on the Atlantic coast of North America."

Before this report was published these gravel deposits were visited by Prof. N. S. Shaler, who was fortunate enough to find a characteristic specimen, but not *in situ*. I also found one, likewise in the talus. Of these specimens, Professor Shaler says, "Although the whole face of the escarpment is in motion, creeping slowly under the influence of frost and gravity towards its base, it was difficult to believe that these specimens, found about twelve feet below the top of the bank, had travelled down from the superficial soil."

Continuing my own researches, in 1877, I made a second report

on the occurrence of these implements, and re-affirmed my conviction that in the specimens of artificially chipped pebbles, from these gravel deposits, we have evidence of man's presence at an earlier date than the supposed advent of the Indian; and referred them geologically to the glacial epoch, in accordance with the writings of Professor Cook, state geologist of New Jersey, who had pronounced these gravels as of glacial origin.

This, briefly, is the history of my own labors in this field; labors continued to the present time and with results that have invariably confirmed my impressions, as I have outlined them.

But admitting that a given class of stone implements is characteristic of a given deposit of gravel, and I think we must admit this now, what is the geological history of this deposit? Is it too recent to be of special import, or too ancient to be of archæological significance? Both views have been held, and neither proves tenable. That the former view should have found supporters is indeed strange. Certainly there is now no movement of the gravel by the river, whatever its condition or freshet stage; and certainly, if these rude forms were of identical origin with common Indian relics, then rude and elaborate alike,—jasper, quartz, porphyry and slate together; axes, spears, pottery and ornaments, all of which are found upon the surface, should have gradually become commingled with the gravel, even to great depths. Any disturbance that would bury one, would inhume alike the various forms of neolithic implements. Such, however, is not the case.

How old and not how recent are the Delaware valley, or, as they are now known, Trenton gravels? This, it is all-important, should be definitely determined. Until recently, there has been the widest range of opinion upon this point, and so great an antiquity claimed, that it was wholly incredible that man should then have lived. How true it is, as Professor Morse has tersely remarked, "Man, profoundly interested in his origin and antiquity, finds himself hampered in his investigations by the opinions and prejudices that have grown up with him. He finds it well-nigh impossible to step outside of himself and regard himself as a mammal among hundreds of other species of mammals."

Depending upon others for my geology, under the circumstances mentioned, it can readily be understood why I was often so sadly bewildered. It was not only an instance of many men of many minds, but occasionally the same individual with numerous opin-

ions. Archæological investigation, under such circumstances, was an up-hill task, the path to the truth being blocked by the obstacles that ignorance, prejudice and hasty conclusions heaped about it; but all the while, the gravel-beds themselves were inexorable and continued to yield evidences of man in spite of the interdictions of the baffled prophets.

A clear light was finally thrown upon these implement-bearing gravels, as the result of a careful study of them, from a geological standpoint, by Rev. G. Frederick Wright, who, as we have already seen, has determined the relationship of all such deposits, lying immediately south of the terminal moraine, to that greater deposit, and so given us approximately, their own age and connection with the last glacial epoch.

It is not necessary to give in detail, the conclusions reached by Mr. Wright. Suffice it to say, that he shows these gravels to be the last important result of the glacial epoch, the direct result of the melting of the glaciers, as they retired northward; and that while this was in progress, the rude implements of palæolithic man were lost and embedded in them.

Admitting this, how long ago did it take place? How great an antiquity does it imply? In this matter, Mr. Wright has been very generous, for which we are duly thankful, for the archæologist has an almost insatiable appetite, never yet having had his fill of ages.

Concerning the antiquity of palæolithic man in North America, Mr. Wright has remarked as follows:

"A word may properly be said with reference to the bearing of these facts upon the date of man's appearance in America. In the first place, it should be observed that, to say man was here before the close of the glacial period only fixes a minimum point as to his antiquity. How long he may have been here previous to that time must be determined by other considerations. Secondly, with our present knowledge of glacial phenomena, the date of the close of the glacial period is regarded as much more modern than it was a few years ago. Sir Charles Lyell's estimate of 35,000 years as the age of the Niagara gorge, which is one of the best measures of post-glacial time which has yet been studied, is greatly reduced by what we now know of the rate at which erosion is proceeding at the falls. Ten thousand years is now regarded as a liberal allowance for the age of that gorge. But, finally, the term "close of the glacial period" is itself a very indefinite expression. The glacial period was

a long time in closing. The erosion of the Niagara gorge began at a time long subsequent to the deposit of the gravel at Trenton and at Madisonville. Between those two events time enough must have elapsed for the ice-front to have receded a hundred miles or more, or all the distance from New York to Albany; since only at that stage of retreat would the valley of the Mohawk have been freed from ice so as to allow the Niagara River to begin its work. The deposits at Trenton and Madisonville took place while the ice-sheet still lingered in the southern watershed of New York, Pennsylvania and Ohio," and in a letter to me, bearing upon this quesion, he has kindly added, "you have got all the time you need, so far as I can now see." This is certainly encouraging! There was a time when, to all appearances, American archæology would have to be squeezed into the cramped quarters of ten thousand years; but we are pretty sure of twenty or even thirty thousand now, in which to spread out in proper sequence and without confusion the long train of human activities that have transpired during prehistoric time.

Mr. McGee, at the last meeting of this Association, in giving the results of his studies of the Columbia formation, remarks as follows: "It has been inferred from the relation of the Columbia formation to the terminal moraine and the drift-sheet which it fringes, that the older deposit represents a period of quaternary cold, much earlier, much longer continued, and accompanied by much greater submergence, than the epoch of cold represented by the newer deposits; and it has been inferred from the relative erosion of water-ways since the two deposits—Columbia and latest glacial—were formed that the interval of mild climate and high level of the land between the two epochs of cold was from three to ten times as long as the postglacial period. These inferences are fully sustained by a long series of observations extending over three years of time and many thousand square miles of area."

If then, we accept the most moderate estimate of the length of postglacial time, some six thousand years, we have of interglacial time (*i. e.*, between the first and second epochs) from eighteen thousand to sixty thousand years, and to this, as I understand the matter, must be added, the long stretch of time during which the second epoch of cold continued. Assuming, therefore, that geologists have made no mistake, archæology has time enough and to spare. At no time was the continent uninhabitable, however thick

and wide-reaching the ice, or deeply submerged the lower lying areas. Still there was land enough for mammalian life in all its glory, and it flourished at the very foot of the advancing ice-sheet, and reëntered every tract as the glaciers withdrew. Then we had the mastodon and mammoth, reindeer and bison, musk-ox and moose and Man familiar with them all.

In November, 1887, Mr. McGee presented to the Anthropological Society of Washington, a communication on "The Conditions of Accumulation of the Trenton Gravels." As it gives the clearest description of the geological conditions of the neighborhood, I propose to quote freely from an abstract of the paper kindly furnished me by the author.

Mr. McGee says: "There are, in the vicinity of Trenton, N. J., two distinct gravel deposits widely different in age. The first is a mass of current-bedded pebbles, cobbles, bowlders and coarse sand, generally graduating upward into a homogeneous loam or brick clay containing rare bowlders; the deposit rises to altitudes of perhaps two hundred and fifty feet in the latitude of Trenton, covers the surface generally as a mantle of variable thickness up to fifty or sixty feet and is sometimes fashioned into terraces—through one of the best examples of which the Delaware river has cut a moderately broad gorge in the upper part of the city of Trenton; the brick clays and gravel deposits along the Delaware river . . . the Columbia formation of the present author (McGee) . . . represent a sub-estuarine or submarine delta of the Delaware river, together with associated littoral deposits formed during the earlier epoch of cold of the Quaternary when the land in the latitude of Trenton was submerged two hundred and fifty feet or more.

The second gravel deposit is confined to an irregular area of the lowlands on both sides of the Delaware river above its great bend at Bordentown. It is composed of pebbles and cobbles (most of which are well rounded), together with scattered bowlders, embedded in a scant matrix of sand, loam and silt; the surface of the deposit is generally horizontal save where cut by recent drainage and its base is irregular; its maximum thickness reaching perhaps forty or fifty feet. It is evidently water-laid, though its bowlders appear to be ice-dropped, and it unquestionably is the southernmost extension of the overwash gravels from the terminal moraine formed during the later epoch of cold of the Quaternary when the

land was depressed as far southward as Philadelphia. It is to this deposit that the name 'Trenton Gravels' has been applied, and its interest to anthropologists lies in the fact that palæolithic implements are abundantly embedded within it.

The configuration and structure of the Trenton gravels alike indicate that they were deposited within and practically filled an estuary of the Delaware river contemporaneous with the later northern ice sheet and the hypsometric and geographic distribution of the deposit indicates the geographic conditions existing above the head of the Delaware Bay at that period. Restored in accordance with the testimony of the Trenton gravels the Delaware Bay of late quaternary time is transmuted from its present condition to a narrow tidal river, similar to the lower Hudson, extending from the terminal moraine to Trenton, and there expanding suddenly into a broad estuary analogous with that of the Susquehanna at its embouchure into Chesapeake Bay; indeed, the ancient Delaware Bay so closely resembled the present Chesapeake Bay that the latter conveys a definite conception of the former. The depression of the ice-burdened land extended southward barely to Philadelphia, and thus the tidal waters occupied a considerable area similar to the expanded head of Chesapeake Bay. Into this ancient Delaware Bay the great river, fed by the melting ice sheet, swept its detritus to be distributed by the waves and deposited in horizontal layers; and during the seasons of most rapid melting, ice floes formed nearer the margin of the glacier, bore the sand, pebbles and bowlders collected in the upper reaches of the river into this bay and there they floated in the currentless waters until they dropped their burdens, just as do the smaller ice floes in the Chesapeake Bay of to-day; while the finer detritus was mainly deposited in the upper reach of the river as is the case to-day in the Hudson. Meantime, the northern ice was a hundred miles away and did not prevent primitive man from assembling about the low and hospitable shores of the miniature sea which was probably the home of fish and fowl just as Chesapeake Bay is now the haunt of myriads of ducks and geese, and a famous fishing ground; and over the bosom of the bay, little affected by tide because of its distance from the ocean, and little disturbed by waves because of its shoalness, palæolithic man may have floated on the simplest craft or even have waded in the shallow waters, as either primitive or civilized man might in the modern Chesapeake. These are the

conditions under which the Trenton gravels were accumulated and the presence of contemporary man is attested by the examples of his handiwork in all horizons of the deposit.

It is significant that all [nearly all—C. C. A.] the palæolithic implements found in the Trenton gravels are of like material—*i. e.*, a variety of argillite—and that natural pebbles of this material are rare in the formation. It occurs *in situ* . . . . not far from the Quaternary ice margin, and bowlders of it are occasionally found in the deposit about Trenton, but its occurrence in the form of pebbles is so rare as to indicate that the implements must have been manufactured at a distance and carried by human agency to the ancient Delaware to be lost beneath its waters. It is significant, too, that the demonstrably artificial objects are least abundant toward the base of the deposit which was laid down before the geographic conditions above indicated were fully developed and that they increase in abundance upward culminating in the superior portion of the deposit formed when the geography of the ancient Delaware most closely approached that of the present Chesapeake; finally, it is significant that the distinctive palæolith found within the Trenton gravel are also found on the adjacent surface made up of the older (Columbia) gravels associated with implements of more modern type, but that they are exceedingly rare over the surface of the Trenton gravels themselves upon which the more modern implements are common.

It should be noted that by study of the Trenton and Columbian gravels in conjunction with investigations of the terminal moraine and other glacial deposits farther northward, the Quaternary history of the region about Trenton has been elucidated. It has been ascertained that the Quaternary period of the geologists comprised two great epochs of cold . . . . and it is noteworthy that all of the remains of palæolithic man thus far authentically reported from the Quaternary deposits of eastern United States have come from deposits from the later ice action."

Why should we abstain from reconstructing a picture of the past from the fossils characteristic of a given geological horizon, simply because man must be included? To deny that any record is read aright because man figures in the past with extinct animals is the height of absurdity; and yet, overwhelming as is the evidence, intelligent people still claim a Scotch verdict of "not proven," must, at least, be given. I go, myself, still farther and claim that

the Delaware Indians witnessed in New Jersey the extinction of the mastodon; evidence to this effect tending to show not so much the very recent destruction of the mastodon in New Jersey, as that the Indian has a very respectable antiquity. With the disposition to modernize everything in connection with the Indian, as is now so popular, I have no patience. To claim that every artistic relic exhumed from the mounds is the handiwork of Europeans, or indicates an association with a superior race, is but a cheap and not creditable method of explaining away the beautiful objects that have been taken from many of these wonderful earthworks.

When my learned friend Dr. Brinton, in addressing this Association, a year ago, remarked, "To me the exceeding diversity of languages in America and the many dialects into which these have split, are cogent proofs of the vast antiquity of the race, an antiquity stretching back tens of thousands of years. Nothing less can explain these multitudinous forms of speech," — I could understand him, thinking as I did, of palæolithic man, and the then even more remote races of the Pacific coast; but when this same author states, as his opinion, "The uncertainty which rests over the age of the structures at Tiahuanaco is scarcely greater than that which still shrouds the origin of the mounds and earthworks of the Ohio and Upper Mississippi valleys. Yet I venture to say that the opinion is steadily gaining ground that these interesting memorials of vanished nations are not older than the mediæval period of European history. The condition of the arts which they reveal indicates a date that we must place among the more recent in American chronology. The simple fact that tobacco and maize were cultivated plants is evidence enough for this," — I am at a loss to discover any valid basis for such a conclusion.

If, as Brinton assumes, the mounds of Ohio were all erected during the past few centuries, what of the "tens of thousands of years" during which the language or languages of the mound-builders was in course of construction and subsequent subdivisions into "multitudinous forms of speech." Is it logical to suppose that, during the immense lapse of time demanded by Dr. Brinton for the formation of languages, these people did nothing, advanced nothing and became capable of building an earthwork only at the very close of an enormously long career? I cannot think it. That there are mounds in Ohio that date far back of any historic tribe of Indians, has, I believe, been fully demonstrated by the cautious and exhaustive explorations conducted by Professor Putnam and Dr. Metz.

Let us return to the consideration of early man in New Jersey and to his association with extinct mammals. That careful student of the subject, Rev. Samuel Lockwood, has given us a delightful account of the discovery of a mastodon in an old beaver meadow, and with his conclusions upon the subject, I will dismiss this phase of the question of early man. Says Dr. Lockwood: "Two facts have much impressed me — the great geological antiquity of the mastodons as a race, and the very recent existence of the individual we are discussing. The race began in Miocene time; this individual lived in the Quaternary age, and well up into the soil-making period. . . . Though the race came before those great castors now extinct, this individual was contemporary with the existing beaver, and doubtless with the aboriginal man. . . . It is plain that the mastodon came into what is now New Jersey ere the ice-sheet began. It receded south before it. It followed the thawing northward, and so again possessed the land. It occupied this part of the country when its shore-line was miles farther out to sea than it is to-day. Here it was confronted by the human savage, in whom it found more than its match; for, before this autochthonic Nimrod, Behemoth melted away."

Having made clear, I trust, what is meant by palæolithic man, and shown also, that he *was* a fact and *is* not a fancy, the question naturally arises, What was his fate? Did he, like the mastodon, become extinct, or has he descendants still living on this continent? There is opened here a wide field, but alas! with so few landmarks and these but ill defined, that the student is much in the position of the mariner when under clouded skies and without a compass.

There has been some speculation and a few bold assertions concerning the relationship of *Homo palæolithicus* to existing races, but I am not aware that any statement has been made, wherein the few facts in our possession are claimed to afford conclusive or presumptive evidence. Certainly so far as my own experience goes, the inference I drew from the character of the stone implements does not accord with the crania found in the Trenton gravel. These are not crania of Eskimos, and I had long inclined to the supposition that to these people might be referred the ruder forms of stone weapons, such as we have seen are found in the gravels of the river valley. On the other hand, the three skulls referred to are unques-

tionably different from those of the known tribes of Indians of the Delaware valley.

Rev. B. F. De Costa has, under the title of " The Glacial Man in America," published a very thoughtful paper in which he endeavors to show, from historical data, the plausibility of the view that the Eskimo now represents this most ancient of America's races.

This author states that " whatever may be concluded ultimately respecting the antiquity of the Delaware flints, it is quite apparent that the red-man found in America at the period of its rediscovery by Cabot, Vespucci, and Columbus, was not the descendant of any glacial man. No line of connection can be made out. This continent does not appear to have any Kent's Hole like that at Torbay, affording a continuous history, beginning with the cave-bear and ending with ' W. Hodges, of Ireland, 1688,' " and again," however man may have reached America, the theory that the Indian peoples sprang from any glacial stock seems untenable. This then, necessitates the inquiry respecting the subsequent history of the primitive inhabitant; otherwise, what became of him?

That a people corresponding in the main to the supposed glacial man once dwelt as far south as New Jersey has been agreed by various writers, without any reference to the contents of the glacial deposits, of whose existence they did not dream. When, for instance we turn to the Icelandic Sagas relating to America, it becomes apparent that the Esquimaux once flourished low down upon the Atlantic coast." And yet again, quoting from the same essay, we find the statement, "The so-called aboriginal red-man is comparatively a modern, although the author of 'Leaves of Grass' asks concerning 'the friendly and flowing savage,' is he 'waiting for civilization, or past it and mastering it?' However this may be, he is wandering over the graves of peoples who left no record of their exploits, either in the continent where they sprang into life or where they died. . . . In New England he must have succeeded the people known as Skrællings. Prior to that time, his hunting-grounds lay toward the interior of the continent. In course of time, however, he came into collision with the ruder people on the Atlantic coast, the descendants of an almost amphibious glacial man. Then the coast-dweller, unable to maintain his position, retreated toward the far north. The northward movement, however,

may have been voluntary in part. During long ages passed in the companionship of the glacier, the race must have acquired that taste and fitness for boreal life which clings to the native of the north to-day, and which makes the Greenlander feel that his country is the most beautiful in the world."

Let us see now to what extent these statements of Mr. De Costa are borne out by the traces of early man in New Jersey. Do they or do they not lend probability to them? I have given you the evidence, so far as gathered of strictly palæolithic man, living in the valley of the Delaware during the last epoch of cold. It consists mainly of the rudest known forms of large stone implements, made of argillite of flinty hardness and breaking with a conchoidal fracture. Its peculiarities as a mineral have already been stated, based upon its examination by Mr. Wadsworth. Now it happens that just as the occurrence of surface quartz veins, near Little Falls in Minnesota, proved the first available locality for that rock, so desirable for making implements, as the ice-sheet withdrew; so, in the Delaware valley, a few miles north of Trenton, argillite occurs in place and likewise offered the first available mineral for effective implements other than pebbles, and these were largely covered with water and not so readily obtained, as at present; while the dry land of that day, the Columbia gravel, contained almost exclusively, in this region, small quartzite pebbles an inch or two in length.

If the palæolithic implements were strictly confined to the gravel deposits, like fossils in the underlying marl-beds, then, as it seems to me, the Eskimo theory would fall, and we could only conclude with Dawkins that "we cannot refer them (*i. e.*, the palæolithic folk) to any branch of the human race now alive;" but, as a matter of fact, there is no such break — no evidence of an hiatus of greater or less duration between palæolithic man and the Indian. The former continued to dwell here until the last pebble of the great gravel deposit had been laid down, and possibly into the soil-making period, but not now, as palæolithic man. Whether the change wrought by the alteration of climate, and its influence on the fauna and flora, had to do with it, or through other influences it was effected, none can tell, but the significant advance to the manufacture of more specialized implements took place; the rude argillite palæolith, the same in form the world over, giving way to spears and other definite forms. The form of the product altered, but the same material, argillite, continued in use. There was no pot-

tery, no polished stone, little if any attempt at ornamentation; still, when we compare these later objects of argillite with the earlier and original patterns, we see what a tremendous forward stride had been made. Had it aught to do with the acquirement of power of speech? as argued by Mr. Hale, that the "speaking man" is a descendant of the "speechless man" of the River drift. But great as the change is, it is insignificant when compared with the handiwork of the Indian — of his handiwork prior to any European contact.

On what grounds can this dissociation be based? Does mere rudeness in the fashioning indicate a difference of origin? Why may not the spearpoints of argillite be the work of Indians as well as similar objects made of jasper? These are questions invariably asked, and however satisfactory the replies have been to myself, they have not proved so, in all cases, to others.

The fact that these rude spearpoints occur upon the surface of the ground is with many an insuperable objection to any claim to significant antiquity; these objectors forgetting the while that there has been habitable surface soil in this region — New Jersey — for a much longer period, than man's first appearance on earth; even in the Garden of Eden.

Let us consider the two questions to which I have referred.

Does mere rudeness in the fashioning indicate a difference of origin? Of itself, certainly not. There are Indians who still make, or did very recently, implements far ruder than the least finished palæolith; and implements of essentially palæolithic character occur wherever ordinary Indian relics are found, but this neither implies that the Indian was a palæolithic man, or that the oldest of these objects, found in gravel deposits was the handiwork of Indians. So long as this confusion exists, so long will American archæology be an unsolvable problem. The telling fact with reference to these argillite spearpoints is that they are not, in the same sense as jasper arrowheads, surface-found implements. They occur also, and even more abundantly beneath the surface-soil.

The celebrated Swedish naturalist, Peter Kalm, travelled throughout central and southern New Jersey in 1748–'50, and in his description of the country remarks: "We find great woods here, but, when the trees in them have stood a hundred and fifty or a hundred and eighty years, they are either rotting within, or losing their crown, or their wood becomes quite soft, or their roots are no

longer able to draw in sufficient nourishment, or they die from some other cause. Therefore when storms blow, which sometimes happens here, the trees are broken off either just above the roots or in the middle or at the summit. Several trees are likewise torn out with their roots by the power of the winds. . . . In this manner the old trees die away continually, and are succeeded by a younger generation. Those which are thrown down lie on the ground and putrefy, sooner or later, and by that means increase *the black soil*, into which the leaves are likewise finally changed, which drop abundantly in autumn, are blown about by the winds for some time, but are heaped up and lie on both sides of the trees which are fallen down. It requires several years before a tree is entirely reduced to dust."

This quotation has a direct bearing on that which follows. It is clear that the surface-soil was forming during the occupancy of the country by the Indians. The entire area of the state was covered with a dense forest, which, century after century, was increasing the *black soil* to which Kalm refers. If, now, an opportunity offers to examine a section of virgin soil and underlying strata, as occasionally happens on the bluffs facing the river, the limit in depth of this black soil may be approximately determined.

An average, derived from several such sections, leads me to infer that the depth is not much over one foot, and the proportion of vegetable matter increases as the surface is approached. Of this depth of superficial soil probably not over one-half has been derived from decomposition of vegetable growths. While no positive data are determinable in this matter, beyond the naked fact that rotting trees increase the bulk of top-soil, one archæological fact that we do derive, is that the *flint implements* known as Indian relics belong to this superficial or "black soil," as Kalm terms it. Abundantly are they found on the surface; more sparingly are they found near the surface; more sparingly still the deeper we go; while at the base of this deposit of soil, the *argillite* implements occur in greatest abundance.

Here, then, we have the whole matter in a nut-shell. The two forms were dissociated until by the deforesting of the country and subsequent cultivation of the soil, except in a few instances, they became commingled.

Perhaps the most important discovery bearing upon the question of the descendants of palæolithic man is that of Mr. Hilborne T. Cresson who has found, in the alluvial deposits at Naaman's Creek,

in Delaware, traces of pile-structures, upon which we may presume that a rude fishing people had erected their houses. A detailed description of these remains and the objects found will be given in due time in the publications of the Peabody Museum, but I may lay stress upon the character of the stone implements dredged from the mud about the piling. At two of the structures or "stations," Mr. Cresson finds only argillite implements, many as rude as some of palæolithic types, and a large number of those long, slender spearpoints to which I have already referred. In a third "station," there is a mixture of these forms with others of quartz, jasper and other silicious mineral, with traces of rude pottery.

These discoveries certainly bear out the suggestion I advanced years ago of an intermediate period of human occupancy of our Atlantic seaboard. Here, on the Delaware river, as Lockwood found in the shellheaps of Keyport, New Jersey, and the surrounding country, occur both jasper and argillite, but not so associated as to demonstrate that both minerals were in use at the same time, or used by the same people at different times. On the contrary, the conclusion reached by every competent investigator has been that implements made of argillite antedate those of jasper; and this single impression of many unbiassed students goes a long way toward proving the essentially correct character of these impressions.

Negative evidence of the soundness of this view is had in the character of the sites of arrowmakers' open-air workshops, or those spots whereon the professional chipper of flint pursued his calling.

In the locality where I have pursued my studies several such sites have been discovered and carefully examined. In no one of these workshop sites has there been found any trace of argillite mingled with the flint-chips that form the characteristic feature of such spots. On the other hand, no similar sites have been discovered, to my knowledge, where argillite was used exclusively. The absence of this mineral cannot be explained on the ground that it was difficult to procure, for such is not the case. It constitutes, in fact, a considerable percentage of the pebbles and bowlders of the drift, from which the Indians gathered their jasper and quartz pebbles for working into implements and weapons.

If the absence of argillite from such heaps of selected stones is explained by the assertion that the Indians had recognized the superiority of jasper, then the belief that argillite was used prior to

jasper receives tacit assent. If, however, it was the earlier *Indians* who used argillite, and gradually discarded it for the various forms of flint, then we ought to find workshop sites older than the time of *flint* chipping, and others where the two minerals are associated. This, as has been stated, has not been done. Negative evidence this, it is admitted, but when considered in addition to the positive evidence of position in undisturbed soil, it has a value that must not be overlooked. Sufficient positive evidence to clear away *all doubt* in the minds of many, of the presence of an earlier people than the Indian on the Atlantic seaboard of America will probably never be forthcoming; yet, to the minds of candid inquirers, there is a degree of probability in the interpretation of known facts that closely hugs the bounds of certainty.

This briefly covers the range of evidence, first, that palæolithic man did not become extinct; secondly, that his descendants attained to an advanced degree of culture in the land of their forefathers. What then was this people's subsequent career? Were it not for the three skulls, to which reference has been made, we could still maintain that we have their descendants in the Eskimo, and that they were finally driven north, after contact with the Indians, who, as is conceded by all students, migrated hither, at, archæologically considered, a not exceedingly remote period. The Indian traditions assert that they found the region occupied; and for once, at least, we have evidence which confirms tradition.

However others may be impressed by what I have now presented, for myself, as I wander along the pleasant shores of the Delaware river, seeing it but a meagre stream between high banks, in midsummer; or, in winter, swollen and choked with ice, until these are almost hidden, I recall what time this same stream was the mighty channel of glacial floods pouring seaward from the mountains beyond and picture the primitive hunter of that ancient time, armed with but a sharpened stone, in quest of unwary game. And later, when the floods had abated and the waters filled but the channel of to-day, I recall that more skilful folk who with spear and knife captured whatsoever creature their needs demanded,—the earlier and later Chippers of Argillite.

These pass; and the Indian with his jasper, quartz, copper and polished stone looms up, as the others fade away. His history, reaching forward almost to the present, I leave in the hands of others to record.

PAPERS READ.

An International Language. By Horatio Hale, Clinton, Ont., Can.

The great inventions of our century, which have brought all civilized nations into such near communion,— the steamship, railway, telegraph, and telephone,—require one essential complement. The closer the intercourse between populations speaking different languages, the more the need of some common medium of communication is felt. The sense of this need is naturally not so strong in America as elsewhere; but every traveller in Europe, every mercantile house with foreign connections, and almost every student of science or art, must experience the embarrassment caused by the variety of languages prevailing in the Old World. Since the era of international exhibitions and congresses for all purposes, scientific, philanthropic, political, and artistic, began, this embarrassment has increased so greatly, that many minds have been turned to the discovery of some means of relief.

The first public manifestation of this sentiment has come from a scientific source, entitled to the highest respect. In January last, the American Philosophical Society, of Philadelphia, adopted by unanimous vote a resolution requesting their President to address a letter to all learned bodies with which the Society is in official relations, and to such other societies and individuals as he might deem proper, "asking their coöperation in perfecting a language for learned and commercial purposes, based on the Aryan vocabulary and grammar in their simplest forms; and to that end proposing an International Congress, the first meeting of which shall be held in London or Paris."

Letters have been issued in accordance with this resolution, and there can be little doubt of a favorable response. The initiative proceeds from the oldest of American learned societies, founded by Franklin nearly a century and a half ago, and numbering on its roll of notable presidents Thomas Jefferson, the most scholarly of American statesmen, and Peter S. Duponceau, the father of American philology. This historical prestige and the very large membership, comprising many of the most eminent scholars in both hemispheres, can hardly fail to ensure a favorable reception of its present proposal. No international jealousy can possibly be aroused by the action of an American society, asking for a meeting in Europe.

The President's letter is accompanied and seconded by an able report from a Committee (Messrs. Brinton, Phillips and Snyder) appointed to consider the subject. The report will be everywhere read with interest,

though some of the views expressed in it will probably arouse discussion. The Committee maintain that inflections are relics of barbarism, and that an uninflected (or, in scientific phrase "analytic") language is better adapted than an inflected speech for the expression of thought. This view, it seems, was strenuously opposed by other distinguished scholars in the Society, who preferred the more usual opinion that inflected (or "synthetic") languages evince in their framers a higher mental capacity than appears in the uninflected idioms. But all the members, without exception,— whatever might be their views on this purely theoretical point, — agreed in holding that an artificial language, designed to be a medium of communication among persons speaking many different languages, should be made as simple and easy as possible, both in pronunciation and in grammar, — a proposition which seems too plain to call for argument.

The Committee, it appears, was appointed in the first instance " to examine into the scientific value of Volapük,"—the "world-speech," of which so much has been lately heard,— an artificial idiom, constructed about ten years ago by the Rev. Johann Martin Schleyer, a learned priest of Baden.

The Committee found in this invention " something to praise, and much to condemn." In fact, its merits and its defects lie on the surface, and are evident to any one familiar with the structure of language and with the need for which such an invention is required. As a distinguished English philologist, Mr. A. J. Ellis, has well observed, Volapük " presents a schoolboy's ideal grammar, there being only one declension, one conjugation, and no exceptions." Indeed, if the object of the invention were to relieve the much-suffering schoolboy of the troubles caused by the monstrous absurdities in the structure of the European languages,— the preposterous orthography of English and French, the nonsensical gender-systems of the continental tongues, the torment of the irregular verbs in all the languages, —Volapük would be everything that could be desired. But while avoiding this obvious Scylla of irregularity, the inventor has been led by his great linguistic ingenuity to plunge into an equally disastrous Charybdis on the other side,— a fatal whirlpool of philological complexities. At the outset, we are met by a gross and surprising error in his alphabet,—an error so evident that the able American interpreter of his system, Mr. Charles E. Sprague (author of a "Handbook of Volapük"), is obliged frankly to admit it. The inventor is not content with the five "pure vowel sounds,"—the *a*, *e*, *i*, *o*, *u*, as they are heard in German and Spanish,— sounds which are familiar and easy to every speaker of every European language, and which, with fourteen or fifteen equally universal consonants, would afford an abundant supply of euphonious words for the amplest vocabulary. He introduces besides, in frequent use, the three German impure vowels *ä*, *ö*, *ü*, easy to him as a German, but to most speakers of other tongues difficult and perplexing. The first and most elementary rule of an international speech evidently should be that no sound, or combination of sounds, should occur in it which is not common to all the leading commercial languages of our time.

But the reason of the introduction of so many vowels soon makes itself

apparent. The author had determined to depart altogether from the analytic system of modern European tongues, and to revert to the ancient synthetic structure. He would have inflections of all sorts,—cases, tenses, moods, formative prefixes and suffixes,— every complication which his strong linguistic faculty and that " study of more than fifty languages " which his admiring biographer ascribes to him could suggest. Thus the personal pronouns, I, thou, and he,— in Volapük, *ob*, *ol*, and *om*,— do not, as in most European tongues, stand independently before or after the verb; they are made inflections and suffixes. *Löf*, for example (a word derived from the English verb, and in pronunciation midway between "luff" and "loaf"), is love, and *löfob*, *löfol* and *löfom*,are "I love, thou lovest, he loves." This, we know, was the way in which the Aryan verb was originally constructed,— the *t* in the Latin *amat* and the German *liebt*, and the *s* in the English *loves*, being relics of an ancient personal pronoun. But thousands of years have passed since any consciousness of this derivation survived. Having reopened this primitive and long-forgotten path, the learned inventor proceeds resolutely forward in it. He gives us, in his imperfect tense, the Sanskrit and Greek augment, and employs for his purpose one of his German vowels, *ä*, having a sound approaching that of the *a* in hat; *älöfom* is "he loved." Then he goes beyond his models, and forms his remaining tenses by other vowel augments. The perfect tense is *elöfom*, he has loved; the pluperfect, *ilöfom*, he had loved; the future, *ölöfom*, he will love; the future perfect, *ulöfom*, he will have loved. The passive voice prefixes *p*, and so we get in the future perfect passive, *pulöfom*, he will have been loved. The conditional mood ends in *öv*, and the potential in *ox*; and thus we have *elöfomöv*, he would have loved, and *löfomox*, which a doubting damsel would need for expressing "he may possibly love." Then there is a reflexive form in *ok*, *löfomok*,—he loves himself,— and a frequentative form, made by inserting an *i* after the augment, and indicating a habit of action — *äilöfom*, he was in the habit of loving. Here we begin to discern the real models which the author of this extraordinary composition has followed. The aboriginal American tongues, with their numerous and apt derivative forms, which have charmed Duponceau, Max Müller, Whitney, and many other noted philologists, have naturally attracted so good a philologist as Mr. Schleyer. He goes on to give us their well-known method of word-formation. He takes bits of modifying words, makes prefixes and suffixes of them, and sticks them on the "stem words," in a fashion which would delight an educated Iroquois or an Ojibway. From *smalik*, small, he makes a prefix *sma-*, and joining this to *bed*, which has its English meaning, we get *smabed*, meaning nest. From *gletik*, great, we derive *gle-*,which, added to *zif*, town, gives us *glezif*, great city. From *län*, country, he makes a suffix *-än*, and so we get *Bayän*, Bavaria, and *Kanadän*, Canada.

These specimens show sufficiently the class of languages to which Mr. Schleyer's invention belongs. The Committee of the Philosophical Society incline to rank it with the "agglutinative" tongues of Northern Asia; but it is something more and better than one of these. It is really what philologists style a "polysynthetic" language, of the American class, combining agglutinate and inflective forms in a great number and variety. To say

this of Mr. Schleyer's work is by no means to decry it, but rather, in a certain sense, to exalt it. Max Müller, after a careful study of the Mohawk language, has declared that the framers "of such a work of art must have been powerful reasoners and accurate classifiers." Professor Whitney places the structure of the Algonkin speech, in its "infinite possibilities of expressiveness," above that of the Greek itself. If any votary of Volapük chooses to claim these praises for that speech and its inventor, there need be no demur to the claim.

But these very merits, in the form in which they are displayed in the Mohawk or the Greek, are utter disqualifications for an international speech. The last species of tongue which an experienced linguist would select as a model for such a speech would be one belonging to the polysynthetic or highly inflected class. In fact, the "ideal form" of an international language would be a language without a single inflection. Such are those curious composite idioms which have sprung up spontaneously in various parts of the world as means of intercourse between persons speaking different and difficult languages,—the *Lingua Franca* in the Mediterranean, the "pidgin English" of the Anglo-Chinese settlements, the "Chinook jargon" of our Northwest Coast. English itself, as the Committee point out, is merely such a *lingua franca* or "jargon," in which the slowly coalescing Saxons and Normans learned to speak together, dropping at least four-fifths of their inflections in the process.

The requisites of an international language, so far as alphabet and grammar are concerned, may be very briefly set forth. The chief commercial languages of the world are the English, French, German, Spanish and Italian, and the proposed speech must be made acceptable and easy to the speakers of all these languages. *Its alphabet must comprise no sounds, and its grammar no inflections, which are not found in every one of these five languages.* This simple and essential rule will reduce the alphabet to the five pure vowels and about fifteen consonants, and the grammar to less than half a dozen inflections, all terminal,—a plural form, a past tense, a present participle, a past participle, and possibly one or two other forms. There would be added four or five rules for the collocation of words,—defining the position of the adjective with regard to the substantive, of the adverb with regard to the verb, and of the verb with regard to its subject and object. The whole grammar of the speech, alphabet and all, would not occupy more than two or three pages of the handbook, and its acquisition by any intelligent person would not require more than an hour or two of application. In fact, the international language will be nearly all vocabulary. The grammar will be merely an infinitesimal adjunct to the dictionary, instead of being, as with Volapük, the main feature of the language.

Volapük is taught in "courses" of lessons. In Vienna, we are assured, seven "parallel courses" have been going on, attended by about 2000 persons. In Paris twenty courses are spoken of. In that city, Kerchoffs has published a "Complete Course of Volapük," supplemented by an "Abridged Grammar of Volapük," and by a smaller "First Elements of Volapük." The latter, by the way, is said to have reached its forty-second edition,—a striking evidence of the need which is felt in Europe of an international

speech. One writer, Mr. Iwan Iwanowitch, has undertaken to give us "Volapük in Three Lessons;" but he does this, as Mr. Sprague's Handbook shows us, by dropping or slurring over many of its forms. Mr. Sprague, more reasonably, presents us with a series of twenty-eight "exercises." We are told, triumphantly, that apt scholars have learned to read and write the language, with the aid of a dictionary, in a week. A week! Now let us see what will be the method with the International Speech.

A merchant in Philadelphia receives a letter of some fifty words from Moscow, superscribed "International Language." He has never seen or heard a sentence of this new speech, though he has read of it in the papers. He sends his clerk for a handbook, which he finds comprises two or three pages of alphabet and grammar, followed by a dictionary in the usual twofold arrangement, International-English, English-International. He glances at the first pages, and takes in the few brief and simple rules on a single perusal. He sees that he has merely to look out the words of the letter, as they stand alphabetically in the International-English part, and put them together in the prescribed order. Half an hour suffices for this simple operation. The few inflections give him no trouble, for they are precisely of the sort he uses in his own language. In composing his answer he reverses the process, and picks out the words in the English-International part. This, at the first attempt, will be somewhat slower work; but less than three hours from the time he opens the handbook will see the translation and the reply completed. A "course of lessons" for the International Language will be as needless as a course of lessons for the telephone. To learn to speak it will require merely the getting by heart of a certain number of words. The grammar, such as it is, will "come of itself," in the acquisition of a score or two of simple phrases, or in reading two or three pages of the printed language.

Every one can see that if the movement commenced by the Philosophical Society shall result in the production of such a means of communication, it will bring an enormous gain to commerce, to the convenience of travellers, to scientific correspondence, and to the friendly intercourse of nations. The creation of such a speech, and in particular the preparation of its vocabulary and the exact rendering of this vocabulary into the principal languages of Europe, will be a work worthy of the best scholarship and the finest intellects of our time. It is saying little to assert that the successful completion of this work will form a step in the progress of civilization not less important — perhaps in some respects much more important — than the successful completion of the electric telegraph.

THE ARYAN RACE, ITS ORIGIN AND CHARACTER. By HORATIO HALE, Clinton, Ontario, Can.

[ABSTRACT.[1]]

VARIOUS theories in regard to the primitive seat of the Indo-European or Aryan race have been proposed, some ethnologists placing it in Asia and

[1] This paper is published in full in the Popular Science Monthly for March, 1889.

some in Europe. Many circumstances combine to point to the great Iranic plateau, comprising ancient Persia and Bactria, as the region in which the race first became conspicuous. But every linguistic stock must have had its origin in a single household. There is reason to believe that the first Aryan household had its home at some point in the Deshtistan, or low country of southern Persia, bordering on the Persian Gulf and Indian Ocean. Thence its descendants spread over the Iranic uplands, and finally over Europe, where they conquered and partly absorbed the earlier European populations. The physical and mental traits of the Medo-Persian people, as they are described by historians, may be assumed to represent those of the primitive Aryans. They were a handsome race, tall and well formed, with features of the Greek type. In character they were brave, energetic, and truthful, but cruel, superstitious, servile and unintellectual. Their bravery and their habit of combined action, resulting from their implicit obedience to their rulers, enabled them to overcome the scattered and disunited tribes of Europe, on whom they imposed their language, and their system of hereditary government, the origin of European aristocracy. This, in its nature and essence, was a genuine caste system, previously unknown to those tribes. These pre-Aryan tribes belonged to three distinct races — Semitic in Greece and other southeastern countries, Iberian in the southwest and west, and Uralian in the north and centre of Europe. All these races were higher in mental and moral qualities than the Aryan, and especially in the love of freedom and aptitude for self-government. The modern European populations are of mixed race, but mainly of pre-Aryan lineage. Their capacity for union and their conquering energy are due chiefly to the infusion of Aryan blood, but their finer intellectual and moral qualities come from the pre-Aryan races. These races, during the last three or four centuries, have been gradually regaining their ascendency, and throwing off the debasing system of hereditary government introduced by the Aryan conquerors. To the gradual elimination of the pernicious effects resulting from the conquest of Europe by the cruel, unintellectual, and rank-worshipping Aryan hordes is mainly due the immense advance in freedom, science, and morality made by the European nations in modern times. The opinion of the peculiar excellence of the Aryan languages, in comparison with other idioms, is an error which arose before the principles of comparative philology were well understood. Many languages of other stocks are superior to the primitive Aryan speech in the capacity of expression. The grammar of that speech has many gross defects, indicating a lack of logical power in its framers. The modern Aryan tongues have been gradually working themselves free from these defects.

NOTES ON AMERICAN COMMUNITIES. By ANITA NEWCOMB McGEE, Washington, D. C.

[ABSTRACT.]

AFTER defining Communism as the doctrine and practice of common ownership of property, and discriminating it from socialism to which it

is related on the one hand and coöperation which it approaches on the other hand, the author proceeded to describe the seven measurably successful American community systems, setting forth briefly the causes which led to their establishment, their social, moral and political attitude, their growth, and the circumstances which led to the dissolution of those no longer existing. Special attention was given to the history of American communities within the last twelve years, during which period the subject has not been comprehensively treated by any writer.

Several communities were visited during the past year by the author, and information concerning others was obtained by correspondence with former and present leaders. They are as follows: 1. The German Inspirationist Community of Amana, in eastern Iowa, now in a flourishing condition; 2. The system of communities of the celibate Shakers in the eastern United States; 3. The German Community at Zoar, in eastern Ohio; 4. The German Harmonist (or Rappist) celibate Community at Economy, in western Pennsylvania; 5. The remnant of the French Community at Icara, in southwestern Iowa; 6. The American Perfectionist Community at Oneida, New York, extinct since 1880; and 7. The Bethel Aurora-Community in Missouri and Oregon, dissolved in 1878. Each of these lasted at least thirty years, but only the first is still increasing in membership.

It was pointed out that the characteristic which distinguishes the successful from the numerous unsuccessful and short-lived communities, is that the former labored under great difficulty and often persecution at their beginning and have since been cautious in the admission of new members.

Finally, the author discussed briefly the desirability of community life and the conditions of stability of community organization, and concluded *first*, that community life is acceptable only to individuals of unusual tastes and ideas; and *second* that, partly for this reason and partly because it antagonizes the family relation and runs counter to the tendency of social evolution, community organization is necessarily unstable and evanescent.

EARLY MAN IN SPAIN. By DR. D. G. BRINTON, Media, Pa.

[ABSTRACT.]

THE Iberian peninsula presents points of especial interest to the general anthropologist and also to the Americanist. It is alleged that the most ancient remains of a man-like animal in Europe have been found there, and the affinities of the Basque language with American tongues have been a frequent subject of comment.

These oldest remains belong to the lower Miocene, and, if authentic, would show that at that early epoch, an animal who chipped flints into tools lived on the western shores of the peninsula. Palæoliths of the Chellean type have been found on the slopes of the Sierra Nevada, in caves of the Cantabrian mountains, and in early quaternary river-detritus near Madrid.

An examination of the last mentioned beds proves that at the period of their deposit a large river was flowing in that locality from the northeast. This is one of many evidences to prove that at the close of the tertiary, and probably long after, an extensive land-area occupied the North Atlantic and connected the Iberian peninsula with North America. It is probable that this was the land-bridge used by the first men who wandered over to the American continent.

Neolithic stone implements are not infrequent in Spain and present striking analogies of appearance with those from northeastern America. The megalithic rock-structures, especially those in Portugal, appear to date from Neolithic times.

The Basques are a pure branch of the white race, who once extended over the whole of the peninsula, but are now confined to the valleys of the Pyrenees. Their language has many structural analogies to the Algonkin.

Traits of primitive speech, illustrated from American languages.

By Dr. D. G. Brinton, Media, Pa.

[ABSTRACT.]

The earliest intelligent utterances of man were undoubtedly much ruder than any language now spoken. What they must have been can be ascertained to some extent by a study of the simplest existing tongues. The American languages offer exceptionally favorable materials for this, as they have been so isolated that they retain their primitive traits. The phonetics of these tongues indicate great variability combined with material significance. Both vowels and consonants have fixed meanings attached to them, but are permutable under certain vocal laws. The Tinné and the Cree offer numerous examples of both these peculiarities. Many of the consonantal sounds are double, or, as we should express it, one letter of the alphabet can not be used without another pronounced with it; thus the Tupi cannot utter the sound of *b* without preceding it with the sound of *m*. There does not appear, however, to be a fixed relation between sound and sense in these primitive utterances.

Many of the radicals, both phonetic and syllabic, mean both an idea and its opposite. The explanation of this is that according to what is known as the second law of thought an idea is defined by an equation embracing both the idea and its privative, as $a =$ not not $- a$. In a similar manner primitive speech included under the same sound both the positive and the privative of the idea.

The most marked characteristic of the grammar of these tongues is their effort to express the whole proposition in one word by the process of incorporation. This is believed to be universal in American tongues. These word-sentences were disconnected, and even yet there is no well-perfected

machinery in these tongues to display the relation of the leading and dependent clauses; relative pronouns and conjunctions are unknown.

Many ideas can be expressed only in relation, as an active verb and its object, the parts of the body, consanguinity, etc. Numerals in primitive speech were unknown, as were also grammatical gender and case. Early man appears to have been a visuaire rather than an auditaire, and his main distinction of things was into living and not-living, or animate and inanimate.

On the alleged Mongoloid affinities of the American race. By Dr. D. G. Brinton, Media, Pa.

[Abstract.]

Many writers have asserted that the American race presents various racial traits of the Mongolians, and hence have classed the Americans with the Mongolians or applied to them the term "Mongoloid."

These alleged affinities may be examined either as of language, of culture, or of anatomical peculiarities.

The Mongolian people, taken broadly, speak either monosyllabic and isolating, or polysyllabic and agglutinating languages. The Eskimo has been asserted to resemble the latter, the Otomi the former; but a moderate examination proves the supposed analogies to be remote and insignificant.

In culture there are many parallelisms, but nothing which would justify a belief that the one civilization sprang from or was materially influenced by the other.

The anatomical analogies which have been adduced refer to the color of the skin, the hair, the shape of the skull and the oblique eye; all these, on examination, prove delusive, while some other traits, as the nasal index, are decidedly un-Mongolian. It is also true that those tribes, once said to be peculiarly Chinese-looking, have not proved so on closer examination.

A limonite human vertebra from Florida. By Dr. D. G. Brinton, Media, Pa.

[Abstract.]

A human vertebra from a postpliocene deposit in Florida, the calcareous tissue completely removed and replaced by hydrated peroxide of iron.

Note on certain prehistoric ornaments found in Mississippi. By Prof. R. B. Fulton, University of Mississippi.

[Abstract.]

Exhibition of certain ornaments of a hard jade-like, but reddish material, found near Wesson, Mississippi, about sixty miles east of Natchez.

THE DEVELOPMENT OF THE CIVILIZATION OF NORTHWEST AMERICA. By Dr. F. BOAS, New York, N. Y.

[ABSTRACT.]

THE civilization of Northwest America is not uniform, but three centres may be distinguished which agree fairly with the linguistic divisions: the Tlingit, Tsimshian, Kwakiutl and Salish. Totemism of these groups; their mythologies; their social organization. It is possible to trace the influence of the Kwakiutl upon their neighbors and to show that they have had a material influence upon the civilization of Northwest America. Discussion of their dances. The use of cedar-bark ornaments. The cannibalism is connected with their dances. The Tsimshian, Haida and Nutka adopted these customs from the Kwakiutl. Linguistic and historical proofs of the fact. Probable origin of the use of masks and heraldic columns. The development of arts among the Haida.

Alleged similarity of Asiatic and Northwest coast culture. A similarity of the Kwakiutl, Salish and Tsimshian elements out of the question. It is necessary to study the Haida element and it may be that here a connection exists.

RECENTLY DISCOVERED ALGONKIN PICTOGRAPHS. By Col. GARRICK MALLERY, Bureau of Ethnology, Washington, D. C.

[ABSTRACT.]

DISCUSSION of the publications of Henry R. Schoolcraft, issued in 1853, upon the pictographs of the Ojibwa, giving the impression that they were nearly as far advanced in hieroglyphic writing as the Egyptians before their pictorial representations had become syllabic. Doubts of the accuracy of these accounts had been entertained, hence an expedition was made last summer by Col. Mallery and Dr. Hoffman to five reservations in Minnesota and Wisconsin to learn what might remain on the subject, resulting in obtaining a large number of bark records old and new, showing great pictographic skill and its general use in ordinary affairs of life as well as religious and ceremonial, though Schoolcraft had colored and exaggerated.

Account of an expedition by Colonel Mallery to Maine, New Brunswick, Cape Breton and Prince Edwards Islands and Nova Scotia to investigate the bark records and petroglyphs of the Micmacs and Abnaki, with special attention to the aboriginal nature of the characters first used by French missionaries in 1652, and afterwards published by Father Kauder in 1862, commonly called the "Micmac Hieroglyphics." An important and unique body of rock etchings was discovered at Kejemkoojic Lake in Nova Scotia and petroglyphs near Machias, Maine, also a variety of bark records and traditions relating to them.

Copies of about one hundred of the pictographs, some of original size, and some enlarged so as to be visible, were hung on the walls as illustrations of the paper.

THE ONONDAGAS OF TO-DAY. By Rev. W. M. BEAUCHAMP, Baldwinsville, N. Y.

[ABSTRACT.]

THE Onondagas have almost forgotten their past history. In the past half century there has been a gradual improvement in their condition, but it does not yet rank high. Social and political changes are continuous in the clans and in the government of the nation. There is much mixed blood, though less than is supposed. The white dog feast is now kept without the dog, and there are changes in other feasts. The False Faces maintain some early customs, and form a society. A recent condolence at Onondaga showed old usages, but in a modified form. It was conducted by the Oneidas. Some early kinds of cookery are retained as well as early implements and articles of furniture. Many superstitions remain, but all are not such which have this appearance. The dead feast and the driving off of witches are the most important. Strings of wampum, differing in color, size and number are used in many ceremonies, and were briefly described. They have been partial to silver ornaments, but these have mostly disappeared, and those shown are fair samples of the remaining forms. Changes of life and environment are rapidly affecting the language, as new words have to be formed and old ones abandoned.

THE RARER INDIAN RELICS OF CENTRAL NEW YORK. By Rev. W. M. BEAUCHAMP, Baldwinsville, N. Y.

[ABSTRACT.]

THE grooved boulders of Onondaga and Yates counties are remarkable the grooves being from eight to twenty inches long, and about three-fourths of an inch deep and wide. They are of the early historic period. Other curious relics are the polished slate arrows, some forms of flint implements, stone cups, stone and clay tubes. Some peculiarities may be seen in pipes of different eras, and there are noteworthy differences in copper implements, beads, scrapers, drills and earthenware.

UNFINISHED "BANNER STONES" FROM THE SUSQUEHANNA RIVER. By ATREUS WANNER, York, Pa.

[ABSTRACT.[1]]

IN this paper were described a number of unfinished "banner stones" from a limited region along the Susquehanna river. The specimens were so grouped as to illustrate successive stages in their manufacture.

As determined by the specimens presented, the process of making banner stones consisted in: first, roughly flaking and pecking into shape; second, grinding; third, drilling the hole.

In conclusion the author gave as his opinion that a hafted implement, with a sharp point, was used in pecking the various specimens described.

[1]This paper will be published in the "American Naturalist."

THE PSYCHOLOGY OF DECEPTION. By Prof. JOSEPH JASTROW, Madison, Wis.

[ABSTRACT.]

A FUNDAMENTAL distinction necessary to understand how deception is carried on is that between what is given and what is inferred in perception. We confuse the two as well as make inferences unconsciously.

A perception depends as well upon the nature of the percipient as upon the object perceived. We interpret the unknown by the known; one type of deception occurs when this general rule does not hold good. Another type of deception depends upon subjective causes, the misplacing of attention, lapses of memory, a dominant idea, prejudice and so on. Conjuring and the physical manifestations of spiritualism furnish the most apt illustrations of the process.

SOME OF THE CHARACTERISTICS OF THE YAKUTAT INDIANS OF ALASKA. By Prof. WILLIAM LIBBEY, JR., Princeton, N. J.

[ABSTRACT.]

DISCUSSED briefly the physical characteristics of these Indians from notes made on a trip among them in 1886.

SOME ANCIENT DIGGINGS IN NEBRASKA. By Prof. J. E. TODD, Tabor, Ia.

[ABSTRACT.]

THIS paper gave a description of an ancient quarry, many square rods in extent, which is on the farm of Mr. Isaac Pollard, near Nehawka, Cass Co., Neb. It is clearly the work of aborigines. Oak trees, thirteen to seventeen inches in diameter, grow over it. The object of search seems to have been masses of flint in a layer of carboniferous limestone. It is comparable with the famous pipestone quarry of Minnesota, both in extent and depth of excavation.

ON SOME RECENTLY CHIPPED ARROW-POINTS FROM NORTH CAROLINA. By GEO. F. KUNZ, New York, N. Y.

THE SERPENT MOUND AND ITS SURROUNDINGS (illustrated by lantern views). By Prof. F. W. PUTNAM, Peabody Museum, Cambridge, Mass.

ON ANCIENT ARROWS AND A NEW METHOD OF ARROW RELEASE. By Prof. EDW. S. MORSE, Salem, Mass.

PALÆOLITHIC IMPLEMENTS FROM THE GRAVEL, NEWCASTLE CO., DELAWARE. By HILBORNE T. CRESSON, Philadelphia, Pa. [Presented by Prof. F. W. Putnam. Printed in Proc. Boston Soc. Nat. Hist., Vol. xxiv, 1889.]

CHIPPED IMPLEMENT FROM THE GRAVEL ON THE EAST FORK OF WHITE RIVER, JACKSON CO., INDIANA. By HILBORNE T. CRESSON, Philadelphia, Pa. [Presented by Prof. F. W. Putnam. Printed in Proc. Boston Soc. Nat. Hist., Vol. xxiv, 1889.]

EXHIBITION OF COPPER AND STONE IMPLEMENTS. By WALTER C. WYMAN, Chicago, Ill.

IOWA MOUND-BUILDERS' RELICS. By JOEL W. SMITH, Charles City, Iowa.

WOMAN'S SHARE IN PRIMITIVE INDUSTRY (with lantern illustrations). By Prof. O. T. MASON, Washington, D. C. [Printed in American Antiquarian, 1889.]

MAN DURING THE PALÆOLITHIC PERIOD IN AMERICA. By THOMAS WILSON, Washington, D. C.

ON A REMARKABLE GOLD ORNAMENT FROM THE UNITED STATES OF COLUMBIA. By GEO. F. KUNZ, New York, N. Y.

A REMARKABLE JADEITE TABLET FROM SANTA LUCIA COTZULMALGUAYRA, GUATEMALA. By GEO. F. KUNZ, N. Y.

SOME NEW GROUPS OF EFFIGY MOUNDS: THEIR LOCATION AND SIGNIFICANCE. By Dr. STEPHEN D. PEET, Mendon, Ill.

THE CLAN SYSTEM AMONG THE MOUND BUILDERS. By Dr. STEPHEN D. PEET, Mendon, Ill.

DID THE CHEROKEES BUILD THE SO-CALLED VILLAGE ENCLOSURES OF THE SCIOTO VALLEY? By Dr. STEPHEN D. PEET, Mendon, Ill.

THE MOUNDS IN THE MISSISSIPPI BOTTOM LANDS AS PLACES OF REFUGE FROM HIGH WATER. By Dr. STEPHEN D. PEET, Mendon, Ill.

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VICE PRESIDENT, SECTION I.

ALTRUISM CONSIDERED ECONOMICALLY.

THE primary motive of human action has always been the care of self, this being for man nature's first and greatest law. In his unthinking zeal he has often followed this to a degree unnecessary and consequently harmful to others. In his savage state and especially in his primeval condition, where he was subject, like all the lower forms of life, to the law of the survival of the fittest, he could not consider others' interests because they were so antagonistic to his own. Often one of two must starve, and each would let it be the other one; he did not even become conscious that he was so acting for a very long period of time. It was the progress from a being not human to the being called man when sufficient intelligence had accumulated to make him conscious that he could live and let live. That point was also marked by and synchronous with the acquirement of such weapons and such skill as enabled man to procure food enough to make the starvation of some unnecessary. Then the war for the survival of the fittest, as known to biology, ceased among men. Ever since, so far as there has been a struggle affecting the survival of the fittest, and that struggle continues to the present day in certain ways, it has been of a different sort and one which must not be confounded with the biologic law of the survival of the fittest. Major Powell has admirably shown how the strictly biologic struggle has ceased in man but he has not yet shown, as may be, the character of that struggle, largely intellectual, which still works out certain survivals of the fittest.

Having passed from the point where, if he survive, it must be at the expense of others, man began to recognize and to consider the desires of his fellows, and among others he counted not only his fellows but mythical and supernatural beings. Thus appeared the greatest natural basis of religion. It is not strange, therefore, that religion should have existed from very early times and that it should have taught its votaries especially to regard the needs of others. Its mission was to teach a race whose ancestors had been absorbed for untold ages in caring only for self, to adapt itself to a new environment by learning to care for the wants of others. In caring for others the more powerful soon received superior recognition, so it came to pass that supernatural demands took precedence of the rest. When that point had become clear, men were easily tempted to profess to represent the gods in order that they might share the precedence. In this natural way became established the order of duty which was taught by every religion prior to Christianity, viz.:

1. To the gods and their representatives.
2. To self.
3. To others.

Early Christianity must be credited with changing the order of duty to the following:

1. To its one supernatural being.
2. To all others equally with self.

Even under this improved system, many people are led to make great personal sacrifices in the belief that thereby they are living the noblest life possible to man; when, in reality, as it is the object of this paper to show, their sacrifices are either useless or what is worse, grossly injurious both to themselves and to the supposed beneficiaries.

SELF-INTEREST AND SELFISHNESS.

During all the untold years in which it was a physical necessity to regard self even to the injury of others, our ancestors acquired a predisposition thereto which heredity has brought down the stream of time. As being no longer a necessity, its practice long since became one of the recognized evils of the world. We apply to it the opprobrious epithet of selfishness. There is a better term and one which does not imply a moral quality, for there may be devotion to one's own interests which should not be so characterized.

Egoism is such devotion to one's own interests; it may be proper and it may be improper. The term does not imply either propriety or impropriety. Let the word self-interest stand for justifiable egoism and the word selfishness represent unjustifiable egoism.

Egoism then was once a necessity, and while it was a condition to existence, it was justifiable, whatever its effects on others might have been. When things changed so as not to render egoism a necessity, man was still as prone to practise it as before. He was acting under the acquired impulses of ages. It was an extremely difficult thing for him to repress his egoism; it was perhaps even more difficult for him to understand that he ought to do so. And yet the change of circumstances had produced a change in its moral quality. From the practice of self-interest he had passed to the practice of selfishness and he had so passed unconsciously, for the change was in environment and not in him. The same act that had been a virtue was now a vice. Of course centuries were needed for this idea to develop and to be disseminated, but at length it came. Although the terms were not in use, the differentiation had taken place. The terms came when needed to express existing ideas.

ALTRUISM.

Long after egoism had differentiated into self-interest and selfishness came the idea of doing something for others. Man's powers were then so limited that this was not much. Even when he became capable he was slow to discover it and slower to act upon it. Heredity bound him. To loosen him was the mission of religion. Whatever its votaries may claim as to its history and purpose, the one great and overwhelming power that religion has had upon the world is this — it has developed doing for others. It has turned man's attention away from himself to those not himself. A most excellent term to use for this is *Altruism*,— a term first employed only about fifty years ago by Auguste Comte to signify devotion to others or to humanity. Percy Smith, in his "Glossary of Terms and Phrases," defines it as "the doing to another as one would be done by; opposed to egoism."

Such terms as benevolence and charity have been generally used to cover the idea of altruism, but in the mind of every one benevolence and charity involve the moral quality of goodness. It is of the greatest importance to have a word like altruism which does not imply any moral quality and which covers all we do for

others regardless of the consequences, just as egoism covers all we do for self regardless of consequences or of moral quality.

ALTRUISM THUS FAR INDISCRIMINATE.

That mankind has thus far regarded all altruism as good is undeniably shown by the fact that neither English nor any other language has words to distinguish proper from improper altruism. This distinction has not been well developed. It was early seen that the motives were of importance. If we do something for others it should be with a good motive. The act was declared to be of no subjective value unless the motive was lofty, thus: "Do not your alms before men to be seen of them, otherwise you have no reward of your Father which is in Heaven." Calling attention thus to motives was doubtless a great advance upon the preceding times. This improved form of altruism was, however, indiscriminate. Nothing was said nor implied, in the above precept, as to the character of the persons to whom alms were to be given. Nothing was hinted nor thought of the ultimate effect upon the recipient of giving alms, much less of taking steps to prevent any needing alms. Elsewhere the intimation was that all who were poor should receive, as indicated by the direction "Go and sell all that thou hast, and give to the poor, and come and follow me and thou shalt have treasure in heaven." "He that giveth to the poor lendeth to the Lord." "It is easier for a camel to go through the eye of a needle, than for a rich man to enter into the kingdom of God." Here the extreme of altruism was proposed, but utterly without discrimination as to the objective effect.

NECESSITY OF LIMITING THE MEANING OF ALTRUISM.

Just as all people assume the moral character of benevolence and charity, so there is a disposition to assume that all altruism is good, in other words to use it as a synonym. Some writers of much prominence have not properly treated the subject of altruism, and religious writers especially fail to measure its true character; that is, we see forms of altruism held up as the *summum bonum*, its teachings are said to be almost or quite divine. A professor in Johns Hopkins University has recently in the Congregationalist spoken of altruism as the opposite of selfishness which latter term he also confounds with egoism (and spells it egotism). This is very unfortunate. We shall never work out social problems with such con-

fusion of ideas. Seeing men in such positions treat altruism as always a good thing and seeing them urge its practice without consideration or without limitation has prompted this attempt to distinguish between justifiable altruism and unjustifiable altruism as carefully as moralists distinguish between justifiable egoism (self-love or self-interest) and unjustifiable egoism (selfishness). And right here the moral philosophers must be alluded to. They have been so zealous to destroy selfishness that they have urged the doing of good to others without sufficiently distinguishing between seeming good and the evil effects thereof. They have too much determined the quality of acts by an examination of the motives under which the acts were performed and too little by an examination of the effects produced. They ought long since to have studied the character of altruism.

THE GOOD ACCOMPLISHED BY INDISCRIMINATE ALTRUISM.

For 1,800 years the world has had an altruism which failed to discriminate as to the object, and, as will appear later, altruism has often been carried to injurious excess, and yet we have had about as good general results as could be expected under the circumstances. The early step from justifiable egoism to that which discriminated was a long one. From the mind resting on self to considering the immediate wants of others was a great advance. From altruism performed with selfish motives to disinterested benevolence was another grand advance. The order of human progress doubtless required a long discipline in indiscriminate altruism before men should learn to differentiate it by observing its results. Again, not only man's mental progress but that of life on the earth has been by pendulum beats from extreme to extreme, by action and reaction, until finally the golden mean of Horace has been reached. The shield was neither silver as protested by him who viewed it from the east nor yet gold as viewed by him in the west; but, had not each held and proclaimed his opinion, the truth would not have been reached by either. Progress limps and goes by indirections; but the various steps indicated have been taken and well taken.

To Christianity then, by far the greatest exponent of this indiscriminate altruism, is due the great credit of having taught it and measurably brought the world from selfishness to disinterested benevolence. It matters not that the race might have traversed this path under some other banner, and that many tribes have found

it independently. "Honor to whom honor" permits this willing recognition. Although it overlooked this newer feature it had enough to do for man of a more primary character.

NON-RESISTANCE AND SELF-ABNEGATION ALTRUISTIC.

The most intense manifestation of the altruistic spirit is in non-resistance to evil and in utter disregard of self. How beautiful seem to us those precepts pointing thereto.

"Whosoever shall smite thee on thy right cheek turn to him the other also. If any man sue thee at the law and take away thy coat, let him have thy cloak also. Lay not up for yourselves treasures upon earth where moth and rust doth corrupt and where thieves break through and steal. Take no thought for your life, what ye shall eat or what ye shall drink nor yet for your body what ye shall put on. Consider the lilies of the field, how they grow; they toil not, neither do they spin, and yet I say unto you that Solomon in all his glory was not arrayed like one of these. Take therefore no thought for the morrow for the morrow shall take thought for the things of itself. Give to him that asketh thee and from him that would borrow of thee turn not thou away. Go and sell all that thou hast and give to the poor and come and follow me and thou shalt have treasure in heaven."

WHY NON-RESISTANCE AND SELF-ABNEGATION ARE BUT LITTLE PRACTISED.

And yet however grandly its maxims may ring in our ears, whatever praises we may bestow upon its advocates and whatever satisfaction we may express with the past, the day for this indiscriminate altruism has gone by and we are confronted with present duty. To-day the only man who sells all that he has and gives to the poor is the unfortunate one whom we shut up in the insane asylum. To-day the only one who takes no thought for the morrow is the tramp or the beggar. (The professional beggar has even sense enough to keep a bank account.) Those extremes of altruism, non-resistance and self-abnegation, have been discarded. And why? Let us now recognize the virtue in them and understand also just why they are impracticable.

The virtue of those precepts lies in their power to draw men away from self. Read them slowly—not a selfish motive to be found in them. They remove one the farthest possible from thought of self.

At the time when the degradation of women was greatest, when chattel slavery was so universal that even St. Paul returned a runaway to his master, when political freedom was unknown, when drunkenness and debauchery far exceeded the present, the best thing for mankind was to hold up this extreme of altruism as an ideal and even to declare it divine, which it nearly was in comparison with the evils combatted. So long as no one could point out its defects, its force would be and was very great for good. Through the self-inflicted injuries which the early Christians caused in practising these principles was the tide of human selfishness checked. But the evil of these precepts consisted in their subjective influence being excessive (therefore injurious) and in their utter disregard of ultimate and objective results. He who curbs his own selfish and grasping spirit by taking no thought for the morrow lays himself liable to want (which is perhaps the lesser of the two subjective evils), but the objective effect is more far-reaching and only evil. It acts as an incentive to others to idleness, improvidence and ultimate beggary. He who being smitten on one cheek turns the other cultivates patience and self control, but he leaves his assailant all the more ready to smite the next man he gets mad with. Again, the subjective effect has good in it; the objective effect has far-reaching evil in it. If I imitate the lilies of the field which neither toil to make themselves a shelter nor spin themselves clothes, I may be admired for my assurance and freedom from anxiety, but I shall also be cut down by the first frost of adversity and be ruthlessly swept out of sight by the first snow of winter. Objectively, I shall have set a bad example to weaker minds than mine. They will say "Let us eat, drink and be merry, for to-morrow we die." And the world will have paid dearly for my little exhibition of self-culture.

He, who, sued at law for a coat by a grasping neighbor, peacefully folds in a cloak also, may cultivate some useful feelings in his own breast while inflicting an unwise deprivation upon himself, but the victorious plaintiff has become a meaner man and will bring new suits at the earliest opportunity; if not upon this, upon some other man whom he thinks he can browbeat, and there are plenty of lawyers who will help him to do so. He, who sells all he has and gives to the poor, may, if he is very badly eaten up with greed for money, discipline himself in the right direction, but in selling *all* he has deprived himself of the means of self-support in sickness and endangered the care of his family. But all this of subjective

wrong might be perpetrated to curb a grasping spirit through the loss of property. That, however, which he had no right to do, he has done. He has pauperized the poor. The evil inflicted upon scores and perhaps hundreds is in their lessening of self-respect, the cultivation of indolence, the enfeebling of their already weak determinations, the putting farther away of that day when the poor shall be properly paid for their work, and the fostering of that reckless spirit "the world owes me a living and I am going to have it." If the next rich man does not sell out and distribute soon enough they will thirst for his riches,—perhaps for his blood. If some of his wealth is ill-gotten, as is the case with many rich men, they will consider it all so. In such soil the seeds of communism grow. The advocates of anarchy and the haters of government are found always among the poor.

Now note this most remarkable fact,— that every single precept pointing to non-resistance and self-abnegation while subjectively attractive, ignores the objective and ultimate effect; that is, they all seem to be of benefit to the doer but make not an iota of discrimination as to the effect upon others; while, in fact, as history has shown, and as we are now beginning to know, both are injured; but the greatest harm is done to the supposed beneficiaries.

Self-abnegation is thus as far from virtue as selfishness. The golden mean lies between, where our egoism benefits us but does not sting another and where our altruism benefits others in its ultimate effects without sapping their or our own welfare. Selfishness is shortsighted gratification of base impulses. Self-abnegation is short-sighted gratification of benevolent impulses. Both are impulsive, both are shortsighted and both inflict evil upon others.

CHARITY AND BENEVOLENCE.

A more moderate and acceptable form of altruism goes under these names. They are also valuable in curbing the spirit of egoism and have made many people, both givers and recipients, happy for the time being. "To do good and to distribute forget not, for with such offerings the Lord is well pleased." Again no discrimination is made as to the objects of charity and of benevolence, nor as to the remote and real effects of such action. It seems to have been thus far assumed that no discrimination need be made. The exhortations to charity and benevolence never specify the objects minutely, while in fact, this should be the all-important feat-

ure. In seeming prohibition to any suggestion of discrimination we are told that benevolence should be universal because the Creator "maketh His sun to rise upon the evil and upon the good, and sendeth rain upon the just and upon the unjust." Now in the case of sunshine and rain it would be physically impossible to discriminate. It should also be remembered that the same Creator for the same reason sendeth the lightning and the earthquake to destroy both the just and the unjust. But, what is more to the present purpose, he starves to death those who in summer fail to lay by a supply of food for the winter; He smites with disease those who are too lazy to cultivate cleanliness; and He visits the iniquities of fathers upon *thoughtless* children to the third and fourth generation. Here is a lesson in discrimination of cause and effect not to be overshadowed by a few platitudes about rain.

THE ECONOMIC PHASES OF THE SUBJECT.

But we must turn to consider the economic effects of altruism by means of which we are to distinguish justifiable altruism from unjustifiable altruism. So much of description has been necessitated by the newness of the subject, and even now it is to be feared that those who have never discriminated as to doing good to others, except as regards the purity of motive in the doer, will feel more concerned about the integrity of the precepts that have been dissected than about the analysis of truth. Be that as it may, and it would be a matter of regret to offend the ancient prejudices of any, it is to be hoped that the economic remarks to follow will but substantiate and illustrate the principles already laid down.

Now that we have reached the study of social, political and economic science, we are called upon to analyze the subject, to define our terms carefully, to be sure that we build our sciences on facts, and to state our conclusions clearly. And our conclusions are most hopeful. They are that in doing real and not seeming good to ourselves we also benefit the race; that in doing good to others it is not necessary nor wise that we inflict sore deprivation or indignity upon ourselves; that thrift and wisdom consist in taking a reasonable thought for the morrow; and that in nothing so much should we take anxious thought for the morrow as when appealed to for alms or to assist the needy.

Better that they suffer hunger to-day and be made self-respecting and self-supporting to-morrow, than that they be fed to-day

and then be forgotten to-morrow. We best help others by securing them full justice and by refraining from injuring them either through malice or through giving them that for which they return no equivalent.

I. RELIEF OF THE POOR.

This class does not include the sick, the dependent children nor the insane, but simply those who are more or less of the time idle, who receive but small wages when they work, and who ask, or do not ask but seem to need, financial assistance.

So many have been willing to lend to the Lord (*i. e.*, give to the poor) believing that it was a safe and dividend-paying investment, that for eighteen hundred years this has been the usual mode of relief.

Everybody knows that this has not diminished the number. It was very unfortunately said eighteen hundred years ago, "the poor we have always with us," because the saying of it has helped to make it true. Assuming that we are to have the poor always with us, we shall do little to lessen their number. Had it been said upon the same authority "under the beneficent sway of wisdom the poor shall cease to exist among you," as it was said "the wolf and the lamb shall lie down together," by this time we should have been far nearer the realization.

Early Athens—pagan Athens, if you choose, could boast of having no citizen in want, "nor" says the Grecian historian "did any disgrace the nation by begging." This should have been our motto. With all the resources of this nation, its realization would have been easy had the proper course been pursued. In such a country as ours it is not necessary, but it is a shame and disgrace to have the the poor always with us,—that is, poverty which needs relief. In the presence of millionaires, men owning but a single cottage are poor by comparison. We ought always to have such poor among us but these are self-respecting and happy men. They must never be confounded with those who through defective character sometimes require food, coal or shelter to be provided for them. The latter are intended when allusion is made to the poor.

Now, if anything of social and economic value has been demonstrated in this century, it is that giving food, coal and money to the poor from public funds or even by private charities pauperizes and degrades them. Henry George says that "the poor are growing

poorer." If so, to nothing is it more attributable than to the multiplication of charities. "A city of charities and a city of paupers" is the designation of one of our eastern municipalities.

CAUSE OF PAUPERISM.

How charity becomes the cause of pauperism may easily be understood. The problem has been well worked out especially in England. Henry Fawcett, professor of political economy in the University of Cambridge, in "Pauperism: its causes and remedies," published 1871, says: "those get the largest share of charity—not who suffer most—but who can excite the greatest sympathy. Hence securing charity becomes an art; begging, a profession. Hypocrisy and lies are the principal tools. Those who acquire skill in it frequently obtain greater incomes than those who labor."

A case in Washington, D. C., illustrates this.[1] In August, 1885, a boy of about fourteen years was found regularly begging on Pennsylvania avenue. His mother, healthy and reasonably intelligent, lived in a neat house on a pretty street within three blocks of the capitol. There was no sign of poverty nor of distress about the house, inside or out. The boy had during the sessions of Congress sold papers at the capitol reaping a rich harvest. He limped about with a crutch. People gave him five, ten, or twenty-five cents for a paper and asked no change. When Congress adjourned he could still have supported himself well by selling papers on Pennsylvania avenue; but people there did not pay over five cents for papers as a rule. Still they did give to beggars, especially to those with crutches. It easily appeared that the boy could make more money by begging than by selling papers and so he begged. Even after he had been taken into police court twice he returned to the street to beg. It was only with great difficulty that the writer succeeded in stopping this imposition upon the public,—the sweet, confiding public which is ever seeking to give to strangers because "some have thereby entertained angels unawares." Yes, a public which is too lazy to investigate the effects of its alms-giving and which deserves to be imposed upon.

Now let no one express horror at the character disclosed in this child or rather in his mother who was the real actor. She was no better and no worse than the average citizen. She simply exer-

[1] I made the investigation of it in person and prosecuted it in the police court before Judge W. B. Snell.

cised business sagacity in getting money apart from moral considerations. So do Wall street brokers; so do many men who endow colleges, build churches and send missionaries to "the heathen."

THE CURE OF BEGGING.

The solution of this economic problem is of the simplest. Make begging unprofitable and we never need lecture beggars about their loss of self-respect. When the getting of something for nothing becomes impossible, and never till then, will men cease to endeavor to get something for nothing. When you and every one of you completely discontinue giving alms except to those whose circumstances are perfectly understood, self-respect and other moral qualties will develop in those people without even a word being said to them upon that point. In giving to them *you* degrade society far more than they degrade it by asking. This kind of altruism is a curse in the world. Fawcett said of it:—"England was brought nearer the brink of ruin by the old poor law than she ever was by a hostile army." Meanwhile we should be self-respecting enough to admit that tramps and beggars are not very different from many "respectable" people after all.

INVESTIGATIONS IN GREAT BRITAIN.

Those who are interested to examine the economic results of giving to the poor, in England, Scotland and Ireland, will find plenty of books on the subject. The Encyclopædia Britannica contains a good article under the heading "poor law." See also other encyclopædias.

The Scotch are proverbially thrifty and economical and yet they have been degraded by the poor law of 1845. In some parts of Scotland there is ten times the poverty there is in Ireland. That law gives more relief than England's and the money is regarded as a nice gift. Those who had savings in banks transferred them to others. Careful investigation and even the labor test did not quell the applications in any such manner as did the Irish workhouse. Matters came to such a pass that the fishermen of Wick could not get their nets mended, their former assistants saying they could get a living easier from the parish.

In Ireland there is very little out-door relief, the proportion of Scotland being almost reversed — five in-door to one out-door pauper. In spite of Ireland's unjust land system and high rents the

whole number of her paupers does not amount to one-half those of London alone. The Irish will submit to every privation rather than let friends go to the workhouse, which is the legal mode of relief and is not a charity.

In London many people get relief who could do without it and consider it no disgrace. Industry, economy, temperance and self-restraint would enable most of them to take care of themselves if they would. Hence the work-house is a necessary restraint, being uncomfortable or even disgraceful. They therefore shun it. If they may eat without work in some other way, they will; if not, many of them will work. Why are these people in such condition? It is a duty we owe to society to ascertain what are their thoughts, what the motives that have led them to such lives. If the result is that the vices and injustices and prodigality of the rich have in part induced such results, let it be exposed boldly and fearlessly. If injustice in the wage system and in land tenure is the cause in part, let this also be proclaimed.

INVESTIGATIONS IN THE UNITED STATES.

You will, however, be more interested in some figures from our own experience. The Hon. Seth Low, ex-mayor of Brooklyn, N.Y., presented a paper in 1881 at the eighth national conference of charities and corrections in Boston which Robert Treat Paine calls the corner-stone of relief reform. In it Mr. Low sums up his opinion of the world's experience in giving alms (technically called out-door relief.) Of the supposed beneficiaries he says:—

1. That it saps their habits of industry.
2. That it discourages habits of frugality.
3. That it encourages improvident and wretched marriages.
4. That it produces discontent.

His own conclusion as to what he had himself seen was "that out-door relief in the United States as elsewhere tends inevitably and surely to increase pauperism." Here are some of his statistics:

In Brooklyn, during 1877, 46,350 people were relieved at a cost of $141,207. In 1878 no money was given. This immense number of people which had received aid were left to take care of themselves or to go to almshouse or to hospital. What effect on these institutions did refusing to give to 46,350 people have? In 1877 and 1878 these institutions contained 1,371 people; in 1879, 1,389 people; in 1880, 1,199, and in 1881 only 1,171. What became of

the people that had received the $141,207, in a single year? Mr. Seth Low says: "instead of Brooklyn needing as the result of the abolition of out-door relief an almshouse of mammoth proportions, we find at the end of three years an almost imperceptible increase of sick paupers, but a steady diminution of well paupers; and this, too, in the face of a population in the county growing at the rate of 18,000 per annum." At about the same time similar action was taken in Philadelphia with like results. Cleveland's out-door relief account for six years was as follows:—

1875	to	4,590	families	$95,000.
1876	"	3,094	"	$85,000.
1877	"	2,386	"	$70,000.
1878	"	1,568	"	$32,300.
1879	"	1,550	"	$22,600.
1880	"	1,200	"	$17,000.

In March, 1877, was begun a system of requiring an equivalent for the relief furnished. Work at $1.00 per day was provided every man who being able-bodied applied for assistance. The officials were thoroughly convinced that pauperism had been fostered and increased by the old system.

Cincinnati pursued the same course with good results, except that it issued during ten winter weeks coal by the bushel; but even that was improvident and demoralizing. People who know a city issued coal last winter will count on getting it this winter and will take no other thought on the subject.

Now we know, by experiment, that the wise thing to do is to visit all such people in July and August and induce them to lay by a few cents a week for winter's coal, promising it to them at lower prices. If thus reminded to provide for winter they are less sensible than the squirrel; they must in all fairness to themselves be allowed to suffer discomfiture in winter and be taught by bitter experience. He who gives to the poor under such circumstances may be very benevolent at heart, but his influence is worse than that of a miser who refrains from giving.

A PERMISSIBLE FORM OF ALTRUISM.

What then must we do? Fortunately, our altruistic feelings may be gratified in a manner not harmful to the beneficiaries. Robert Treat Paine of Boston, who has had large experience in

treating the poor, prescribes the following: "Whenever any family has fallen so low as to need relief, send to them at least one friend,— a patient, true, sympathizing, firm friend, to do for them all that a friend can do to discover and remove the causes of their dependence, and to help them up into independent self-support and self-respect." To which it may be added, if that friendly visitor is permitted to give alms, his and their minds are diverted from the great object,— the permanent cure of poverty. It should always be regretted when circumstances seem to demand attention to immediate needs. Put off every possible want till the person can himself supply it in a manly and independent way. Better a morsel with self-respect than plenty with an enfeebled determination to fight the battle of life.

II. ORPHAN ASYLUMS.

Much that has been said of giving alms applies to the treatment of delinquent and dependent children. Moved by the altruistic spirit, and feeling an approving conscience as the result of trying to do good to others, the Christian world has taken up the care of orphan asylums. Children are gathered from the slums of cities and sometimes from pretty good homes into these herding places. Then they are told, as I recently heard from a reverend doctor, how grateful they should be to Christianity for thus caring for them; but the fact again is that, prompted by kind motives, people thus try to do these children good without looking to the results of their acts to see the consequences.

EVILS OF ORPHAN ASYLUMS.

What then are some of these consequences?

1. That moral corruption brought in a little by each child leavens the whole lump.

2. That they are often placed under incompetent teachers to learn book lessons, when in fact their capacities call for manual training instead. (Who ever knew a scholar reared in an orphan asylum?)

3. They are fed in the cheapest sort of a way and clothed in a uniform that causes them to be pointed out always in public as objects of charity and degradation.

4. They are kept in herds and not in families and hence subject to rules and training necessitated by this abnormal life. Often

they are so unfit to live in families that kind-hearted people cannot adopt them.

5. Every delinquent mother and every drunken father now knows that he and she can indulge their vices and get rid of their children. Thousands of widowed mothers learning that they can marry again if not encumbered with children are putting their little ones in asylums. The asylum thus offers a premium to child desertion. Rich people even are living in luxury, while their nephews, nieces and grandchildren are being corrupted in orphan asylums. The niece of a president of the United States was, not long ago, in an asylum, while her uncle, aunt and three cousins, occupied the White House. Such people often give as their excuse that the child was too vicious, or rude, or even homely, to be received into their families. "No one seemed to want him."

THE REMEDY.

Better the humble home of a poor farmer in the west, far better for such children as are unavoidably orphaned, than these unnatural corrals. But this kind of orphans constitutes not over one-fifth of all. The other four-fifths represent indulgence by the asylum founders and managers, towards parents and relatives who wish to shirk responsibilities imposed by nature upon them. With every such indulgence issues moral miasma upon society which festers and reproduces its kind. And all this time people with good motives and benevolent spirits thank God they are not as other men are and proceed to build additional asylums. Better and far better live and die among the Zuñi Indians of New Mexico, having never heard of Christian charity, than to die and leave your orphan child alone in a large city of the United States. In the latter case he goes to an asylum to be swallowed up in the masses. In the former case, although he has lost his own father and mother, he has found many fathers and many mothers all of whom will feel a personal interest in him and responsibility for him, and who will share with him if need be the last pot of corn, and will weep over his grave as if it contained their own flesh and blood. We should need no orphan asylums if we possessed the virtues of the Zuñi.

How to set forth the economic effects of such institutions, and to point out to society the way to make its members rear the children to whom they have given birth, and to show the disastrous ef-

fects of ill-considered altruism is a task which comes within the province of this section of the A. A. A. S.

III. FOUNDLING ASYLUMS.

Here again sentimentalism has contributed money to build asylums, and even more unwisely than in the case of the orphans. An orphan cannot be committed without something being known of its parents, or their circumstances, and without formal papers of transfer. This routine exposes many frauds and leads managers to reject thousands of applicants for admission. Managers like to boast of the cases they have rejected. With foundlings nothing of the sort occurs. The girl whose yieldings to temptation have made her a mother, be she in high life or in low, the intemperate who prefer to use their means for drink to rearing their own offspring, the society people who have boasted that there will be no children in their families—one and all—have but to leave their offspring as naked as little Moses was when deposited at the Nile, either in a vacant lot or upon some handy doorstep. Under cover of darkness all is secret. Either a policeman, or the irate citizen whose doorstep has been invaded, quickly and safely transfers the waif to an asylum. The reminders of sin and folly as well as the burdens of the parents have thus been put far away. Were society organized to encourage this very business it were impossible to arrange it more satisfactorily. But eternal shame should rest upon the weak-minded, benevolent people who by their ill-advised altruism cultivaté such degradation in society. One-tenth of the money spent in detecting and punishing these parents for their unnatural crimes would teach society the needed lesson. More pains than we take to catch a murderer should be spent upon detecting these criminals. Every foundling asylum in America should be instantly disorganized.

IV. INSANE ASYLUMS.

Upon this kind of altruistic effort, also a boast of the age, there are not sufficient data to warrant so severe denunciation. It is proper, however, to call attention to some suspicious circumstances.

1. The collecting and imprisoning of great masses of such people is unnatural and the best authorities advocate breaking up the system by substituting homes and separate buildings.

2. To the non-medical observer it is surprising, that while rapid progress is being made in treating many forms of disease such as are caused by minute germs, so little knowledge is being obtained concerning the nature, causes and cure of insanity. With many physicians, supported by the state in a liberal manner, why are they not bringing forth fruit in this direction? It is said to be because incapable men get places through intrigues and because so much time is spent in routine work.

3. The number of the insane is on the increase. Some of the immediate causes are understood. Is it not certainly of the utmost importance that facts bearing on these points be circulated and that great effort be made to check insanity by rooting out of society the immediate and ultimate causes.

The altruistic work suggested by these questions can have no unjustifiable effects. That which has been performed is more questionable as implied by the changes proposed and upon further examination may prove more unjustifiable. In any event it is plain that doing good to those now insane may not be of half the importance that it is to find means of preventing insanity in the future.

V. BENEVOLENCE IN HIGHER EDUCATION.

It used to be a practice to give not only tuition but even board and clothes to young men studying theology. It was considered that they were preparing to lead lives devoted to altruistic work and that it was therefore desirable, in the case of young men apparently without means, to pay their expenses in theological schools, but it worked so badly that the plan is undergoing change. In the best schools where funds are provided, they are now loaned and a written obligation to repay is executed. In other words the managers of schools of divinity found out that to give to the poor theological student was to lend to the devil,— a very different creditor from the one they had in mind. It was found that this money got used at times for tobacco, for pleasure trips, etc., while board bills were unpaid, and that after it was spent the beneficiaries sometimes abandoned the life-work they had contemplated.[1] But even the loan system is not working satisfactorily. The written obli-

[1] There has been in government employ in Washington, the past eight years, a young man who received such aid for two years previously. He now owns real estate in Washington, but he never preaches.

gation is often lightly esteemed and held to be not binding. The writer's latest information is that but thirty per cent. is paid back. Only when the notes are looked after, as a successful banker looks after his paper, will the system become truly beneficent. It will then have ceased to be a charity. Again let it be said, *do not give something for nothing*, but if you really must do so, then put it into a lottery.

VI. GIFTS TO WORKINGMEN.

There is reserved for the last a notice of the most contemptible form of altruism now known to civilization. It has come to be the fashion for people who have acquired money without giving an equivalent in labor and who wish to indulge in benevolence, to build mission chapels in outskirts of cities, and to furnish them with cheap appliances in order to "save the souls" of the dear working classes. Another form this takes is in furnishing workingmen libraries and reading-rooms, and even in building improved tenement houses. In a score of ways the rich are " doing something for the poor." Now all these things have the same surface appearance of charity as throwing a dime to a beggar. But the fact is that these people have by class legislation or dishonesty gotten possession of wealth created by the poor, and in order to quiet their little consciences, or occasionally in order to enable them to keep up the fraud, they dispense these charities.

Now let it be reported to all such that the workingmen need none of their charities. They cry out for justice, for fair wages for a day's work, for reasonable rents, for a chance to buy house lots which speculators have not pushed beyond honest men's reach, — in short for such a reorganization of legislation and custom as will enable them to labor and to administer upon the entire fruits of their labor, to build and furnish their own chapels if they choose, to establish their own libraries and reading-rooms, to build their own tenement houses and to scorn charity as they now have good reason to scorn such dispensers of charity.

CONCLUSION.

The basing of all so-called charitable and benevolent work upon such principles as have been indicated, or rather the substitution of right-seeking and right-doing (which is but the simple prac-

tice of justice) will require earnest study and a great change in our spirit and methods. Those who in preceding years have here listened to outlines of work adapted for this section as presented by Professor Elliott and Major Alvord should notice that the bounds will be much enlarged if we seek to solve the problems which shall enable us to make our altruism economically beneficial. This certainly should be the case. That we should pretend to be doing good to all men and yet be deceiving both ourselves and them, while really doing harm to both, needs only to be demonstrated to secure our condemnation. And giving alms to show even to ourselves our good motives or in order to indulge our benevolent impulses is certainly the most deceitful form of selfishness, since it appears in the form of altruism, is evil and only evil.

PAPERS READ.

THE USE AND ABUSE OF STATISTICS. By EDWARD ATKINSON, Boston, Mass.

[ABSTRACT.]

I CAN cite no more true example of the use of statistics than the admitted necessity on the part of every merchant and manufacturer to keep a set of Books of Account in which the statistics of his business are registered day by day. In the course of a business life now covering more than forty years I have known more failures in business growing out of the neglect of the use of statistics in this manner: *i. e.*, more failures arising from want of knowledge on the part of the merchant or manufacturer of the actual figures of his business than from any other single cause, and perhaps more from this cause than from all other causes of failure put together. Yet the merchant or manufacturer, who attempts to govern his affairs and to guide his business merely by the application of statistics without applying his own judgment in determining what the figures mean, is sure to fail—whether these statistics are recorded on his Books of Account or are the mere statistics of the trade in which he has been engaged.

Whenever a man is wholly guided in the purchase of materials by the statistics of the crop, say, of cotton, and pays no regard to other conditions he may make a profit for one, two or three years, but he is sure to come to grief sooner or later. This has been the common observation of all who have watched the figures of the cotton crop and the course of prices together for any long term of years. These are commonplace examples of the use and abuse of statistics.

I will limit the short treatise which I present to-day mainly to some examples of the abuse of statistics of a more general kind.

The most conspicuous example of this abuse is to be found in the customary reasoning which is based wholly upon compilations of the rates of wages which are said to be paid in this and in other countries. The greater the truth of these comparisons the greater may be the falsehood in the deductions which are made from them. The rates of wages in all the principal branches of manufacture, in the mechanic arts and in agriculture, are unquestionably very much higher in this country than in any other, with the possible exception of Australia and New Zealand; they are decidedly higher than in Great Britain, but the rates of wages in Great Britain are also higher than they are in Ireland, two parts of the same kingdom, governed under substantially the same laws, taxed under substantially the same system, and subject to absolutely the same system of duties on foreign imports.

It is curious to observe that when the union of Ireland was pending, the manufacturers of Lancashire and Yorkshire presented urgent petitions

against such union, upon the ground that if the two parts of the kingdom were brought under the same tariff the manufacturers of England would be ruined by the cheap labor of Ireland and would be obliged to let the capital invested in Lancashire and Yorkshire go to waste and to construct new mills in Ireland in order to avail themselves of low-priced labor which they then conceived to be cheap labor. There can be no greater fallacy than the conception that low-priced labor is the same as cheap labor; *i. e.*, labor that can be made use of at low cost.

Again, in passing from Great Britain to the Continent,—the rates of wages are lower in France than in England; in Belgium than in France; in Germany than in Belgium; in Italy than in Germany; in Austria than in Italy; in Russia than in Austria; and if we pass on to Asia, lower in China and India than in Russia. What more profound abuse of statistics could there be than in deducing the relative cost of production by a comparison of the rates of wages, which thus vary even in different parts of the same kingdom as well as in different countries.

Again, the rates of wages in the several states of this country vary greatly, although not as much as they do in Europe; yet if one were to attempt to get at the cost of any branch of work by the rate of wages, he would find that the lowest cost of production of any given article would not be found where the rates of wages were lowest, but rather where the rates of wages were highest. This leads to the consideration of the source of wages.

The old conception of wages being derived from a fund of capital previously accumulated has been substantially given up and the wage-fund theory, which even as late as the earlier writings of Mill had been fully accepted by economists, has been proved to be without any substantial foundation in fact.

In place of this false theory which has worked a vast deal of mischief, it is now generally conceded that wages are derived from the product on which the work has been exerted. Hence it follows of necessity that where the conditions are most favorable, *i. e.*, climate, soil, relative humidity, proximity to the material of chief value; or where there is a population bred to the necessary conditions of the work possessing an inherited aptitude for the specific occupation; or where the burden of local taxation is least,— *there* certain kinds of industry will become established under the best and most favorable conditions.

This product being brought into the market is sold at the same price as that which is produced under less favorable conditions; hence when the return is made which is to be distributed in money, as profits and wages, the less number of persons who have been required to do the work under the most favorable conditions receive a larger share, *i. e.*, a larger rate of wages or earnings than those who have been occupied in the same art under the least favorable conditions. Hence it follows that aptitude, skill, capacity, good public credit, favorable conditions, good soil, good climate and the like, yet more a suitable education, tend to the production of all goods and wares at the lowest cost measured in terms of labor and also at the highest rates of wages measured in terms of money.

If then, it is true that the people of the United States possess the widest area of fertile soil, the greatest variety of favorable conditions in respect to climate, the largest supply of ores, coal and timber, the best schools, the lightest taxation, the greatest freedom in interstate commerce and service,—it must of necessity follow that with the exception of a few tropical products or a few branches of industry, like the preparation of tea which is absolutely a handicraft, or the preparation of raw silk which in the reeling is also absolutely a handicraft,—the people of the United States will produce the largest product at the lowest cost and will therefore secure the highest rates of wages or earnings; such is proved to be the fact by statistics which cannot be gainsaid.

The working of these laws may be to some extent retarded or obstructed by legislation; one of the worst cases of injurious or obstructive legislation is the substitution of *fiat* money such as Government notes or other forms of inconvertible paper, in place of true coined money made of metal which carries its own value in its own substance. There can be no better examples of the possible abuse of statistics than an exhibit of the relative conditions of prices, wages and volume of money in circulation for the last twenty-five years, which taken separately wholly mislead the compiler.

I have taken from the Census Volume No. xx the statistics of wages of all the mechanics, engineers, carpenters, machinists and painters which are given separately in all the returns of specific establishments contained in that volume. They constitute a class whose earnings or wages at any given time are more nearly even and more near to the average of all wages than that of any other single class of operatives or working people. They have been much misled by special statistics. For instance, in 1860, reference being made to the Table which accompanies this treatise, their average daily wages came to $1.56; in 1865 their average daily earnings came to $2.34. Their minds were abused by these statistics and they thought they were better off; but when we take the price of food, fuel and materials for clothing corresponding to the average consumption of this class of men and their families, the same quantities of the same kinds which could be bought at 30.95 cents per day in 1860 had risen to 55.69 cents in 1865, so that a mechanic misled by his high wages could yet buy with his 300 days' work only 1261 portions of this food, fuel and materials for clothing against 1572 portions in 1860; after 1870 wages began to decline, but prices went down faster; after the resumption of specie payment in 1880 the rate of wages advanced, but the quality of the money being good, prices still continued to decline; and in 1885 and 1886 this same set of mechanics earned $2.40 a day in gold with which they could buy 2400 portions, or very nearly double what they could get at substantially the same rate of wages in 1865, then paid in depreciated paper.

Unless all the qualifying conditions are taken into view, almost all statistics of wages are absolutely worse than useless.

Again, much importance is attributed to the quantity of money in circulation. A few words may be devoted to this subject.

According to a compilation recently made by the Secretary of the Treasury, the quantity of all kinds of money which was in circulation in 1860 was

$14.06 per capita; in 1865, after the issue of the *fiat* money, the quantity was $33.96 per head; yet, as I have proved to you, the condition of the workman was much worse at this time than it had been in 1860 when the quantity of money in circulation was only $14.06 per head. From 1865, down to the resumption of specie payment in 1879, the quantity of money per capita steadily diminished through the withdrawal or contraction of the paper money. In 1880, after the resumption the quantity of all kinds of money in circulation redeemable in coin, or in coin itself, was $24.08 per head, nominally nearly $10 less per capita than in 1865; yet every one prospered, business was active, the rate of interest was low, the rates of wages were nearly as high as they had been in 1865, while the purchasing power of the money had greatly increased, as the diagram will show.

Since 1880, there has been a steady expansion of the quantity of money in circulation. The instruments of exchange now consist of gold coin or certificates based thereon; silver coin or certificates; legal tender notes and bank notes, all practically redeemable on a gold basis; the amount in use, or available for use, is now $27.25 per capita; yet prices are not as high as they were in 1880, while wages are higher and the purchasing power of wages is greater than ever before.

There are now, however, certain forces in action which tend to diminish the amount of money per capita. The excess of revenue tends to an accumulation of money in the Treasury of the United States and there is a fear lest there should be an undue contraction of the currency. This fear may perhaps be dispelled by a consideration of the actual facts.

The excess of revenue falls into the Treasury either in legal-tender notes which, when in the Treasury, become evidence of so much debt paid; or else the revenue comes in coin with which these notes may be redeemed. This accumulation may go on to the extent of the probable excess of revenue for one year to come, to wit, $125,000,000. It may all consist of legal-tender notes; this accumulation will reduce the currency per capita $2.00 per head. Does it not of necessity follow that there will not be enough? Deduct $2.00 per head from the amount now in use or in circulation, to wit, $27.25, and the remainder $25.25 will exceed the whole volume of currency of 1880 which was sufficient to do all the work that was required of it. It might not therefore be any misfortune if Congress should fail to reduce the revenue and if this accumulation should go on until every legal-tender note should either be liquidated by taxation or be sustained by coin, dollar for dollar, in the Treasury of the United States.

To this end all the statistics now point.

I have thus brought before you in a very concise way the relation of prices, wages, purchasing power of wages, and volume of money per capita at intervals of five years from 1860 to the present time. You are all witnesses to the abuse of statistics in the separate comparisons of rates of wages and prices of goods, on which separate comparisons many adverse conclusions as to the condition of the country have been based. Does not the true use of these statistics wholly consist in grouping them together, ascertaining their relation each to the other and then deducing from these facts such conclusions as they may indicate? In other words, may it not

be held that unless statistics are made use of as a basis of economic reasoning by persons competently trained, they become a mere snare and pitfall, working more harm than good through the false deductions that may be made from them, while, on the other hand, the economist who attempts to reason on the conditions of men in their relation to each other, without regard to the statistics of prices, wages, volume of currency and other elements by which the exchange of services is controlled or measured, will of necessity be a mere theorist whose unsustained hypotheses may or may not come near the mark?

RELATION OF WAGES—PRICES—PURCHASING POWER OF WAGES AND VOLUME PER CAPITA OF MONEY OR CURRENCY IN CIRCULATION AT THE RESPECTIVE DATES GIVEN.

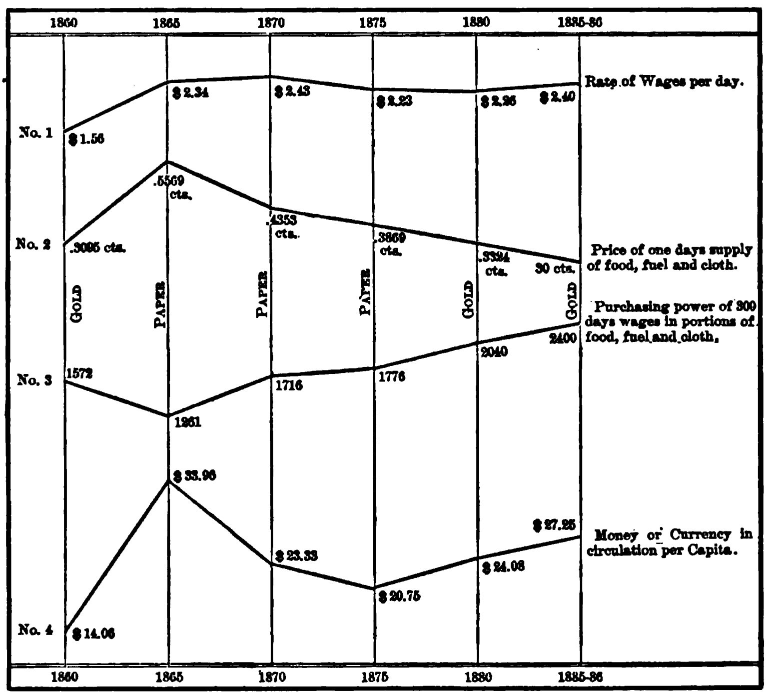

DIAGRAM NO. 1.

No. 1. Average wages of mechanics, such as engineers, carpenters, machinists, and painters, connected with the mills and works treated in Vol. XX, United States Census—establishments in eastern, middle and western states.

No. 2. Average cost of one day's supply of food, fuel and materials for clothing, customarily used by such mechanics, computed at retail prices in twenty shops—ten east and ten west of Buffalo, N. Y.

No. 3. Variation in the purchasing power of 300 days' wages in money expended for equal portions of the same kinds of food, fuel and cloth, as above given.

No. 4. Quantity per capita of coin, convertible bank notes, legal tender notes, in circulation or in use as money at the respective dates.

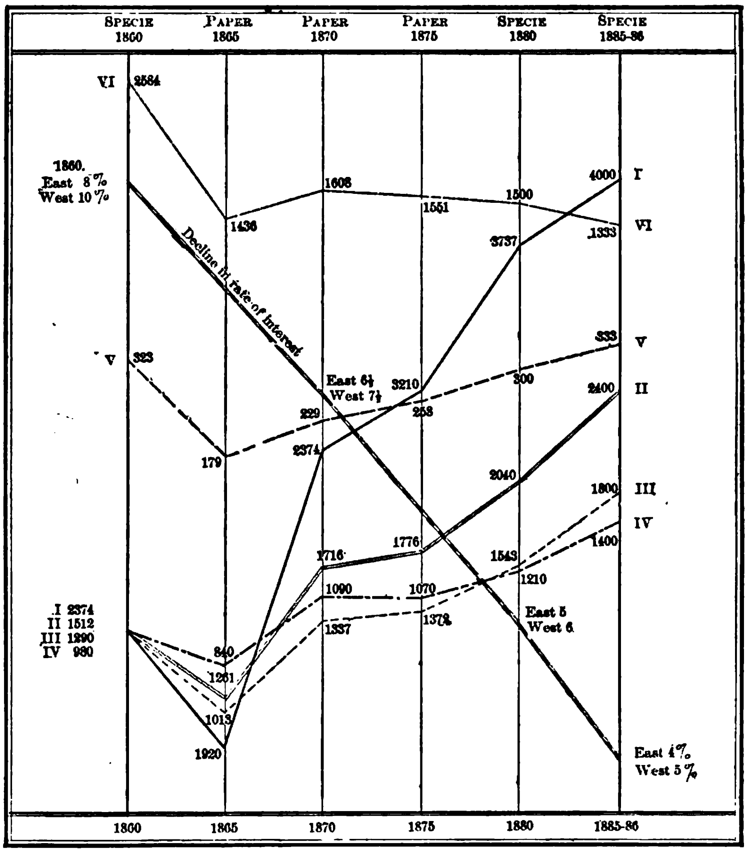

DIAGRAM NO. 2.

The foregoing table has been computed by taking the prices of equal quantities of the same kinds of food, fuel and materials for clothing at different dates, corresponding in kind and quantity to the average consumption of good mechanics in the eastern and middle states. The upper line numbered VI, represents the loss in the purchasing power of the income of ten thousand dollars invested in safe securities. The result is reached by dividing the price of a single portion or day's supply of food, fuel and materials for clothing at the different periods named, into the sum of the interest which could be earned at the different dates on ten thousand dollars invested in the safest manner.

The next line crossing the table diagonally from left to right gives the average decline in the rate of interest on the best commercial paper between the dates named.

The third line numbered V, represents the purchasing power of one hundred dollars of lawful money converted into portions representing a quantity equal to one day's supply of food, fuel and materials for clothing at the respective dates named.

The four lines, starting from the same point numbered in Roman numerals, I, II, III, and IV, represent the purchasing power of three hundred days' wages at the average rates earned, when converted into similar portions or days' supply of food, fuel and material for clothing.

I. Specially skilled mechanics, overseers, foremen and the like.
II. Journeymen, mechanics of average skill and capacity.
III. Operatives in factories of various kinds, textile, wood-working, boots and shoes, metal works and the like.
IV. Common laborers.

The data from which these rates of wages and their purchasing power have been compiled are taken from one hundred establishments reported in volume twenty of the United States Census, compiled by Joseph D. Weeks. In selecting the establishments the writer chose among a very much greater number, one hundred which he had reason to suppose had been in substantially continuous occupation throughout the period from 1860 to 1885–6; or if subject to temporary stoppage, yet for very short periods.

In making these compilations the attempt has been made to devise a multiple standard at the prices prevailing of the actual necessities of life as a substitute for the unit or standard of money, which has varied so greatly between 1860 and 1885–6 in consequence of the issue of inconvertible paper money.

These tables are given as examples of what the writer considers the true use of statistics, in which the variation in prices, in the rates of wages and in the purchasing power of money are compiled and compared.

The common abuse of statistics consists in treating prices and rates of wages without any consideration of the quality or purchasing power of the money in which these prices and rates of wages are given. The depreciation of the legal tender notes of the United States from 1861 to 1879 have utterly vitiated all the statistics of that period relating to prices and wages unless the figures are adjusted to the variations in the value of the currency.

NEED OF A FOREST ADMINISTRATION FOR THE UNITED STATES. By B. E. FERNOW, Chief of Forestry Division, U. S. Department of Agriculture, Washington, D. C.

POLITICAL economy, as the name implies, is that science which teaches us, as a nation, to manage our resources, to avoid waste—waste of energy, waste of resources. In the United States it will be readily admitted this science of avoiding waste has not been much practised; our natural resources, especially, abundant as they were, have been and still are squandered in a manner which must needs frighten him, who has a thought for those that come after us.

Among the most useful resources to a nation of pioneers must have been the supply of material which the forest furnishes, and yet these very supplies so essential to the pioneer civilizer have been not only squandered most ruthlessly and unnecessarily, but the possibility for their natural recuperation — by which the forest excels all other natural resources — has, to a large extent, been wantonly destroyed or deteriorated.

That the farmer who needs the ground for agricultural use should have cleared away the timber growth immoderately, and often imprudently; that the lumberman and private forest owner should have prized the present realization of his forest value more than its higher prospective value, and should have cut and slashed without thought of economy, and, to overcome competition, expensive transportation, etc., should have wasted rather more than necessary, we can perhaps understand, explain and excuse.

But that the government, without any need of a present income, should have so utterly neglected this branch of political economy and failed to perceive its interest and duty in the preservation and continuity of this richest of our resources, seems remarkable.

Our government has failed to recognize the importance of this factor of national wealth in general; and, worse, after having disposed of the largest part of it unconditionally, it has failed to take care of that part which was left in its own hands, as a trust for the people.

To make good the assertion that the forest supplies have been among the most important factors of our nation's wealth, I cite, from the calculations of Professor James, the comparison between the products of the forest and those of other industries as appearing upon the basis of the census of 1880.

"The value of the forest products in that year (1879) was equal to one-third of that of all farm products whatsoever sold, consumed or on hand. It exceeded by nearly one hundred million dollars the total assessed value of all the farming property in the New England States and by a somewhat smaller figure that of the farms of Virginia, North and South Carolina, Georgia, Alabama and Mississippi. It would have purchased at its assessed valuation for the purpose of taxation the entire property, personal and real, of all the citizens of the States: Vermont, Delaware, Florida, Arkansas, Nebraska, Colorado, Nevada, Oregon and of all the Territories besides and still have left a balance nearly equal to the same kind of property rated in the District of Colnmbia.

"If to the value of the total output of all our veins of gold, silver, coal, iron, copper, lead and zinc, were added the value of the product of stone quarries and petroleum wells and this sum were increased by the estimated value of all the steamboats, sailing vessels, canal boats, flat boats and barges plying in American waters and belonging to citizens of the United States, it would still be less than the value of the forest crop by a sum sufficient to purchase at cost of construction all the canals, buy up at par all the stock of the telegraph companies, pay their bonded debts and construct and equip all the telephone lines in the United States.

"This sum of 700,000,000 dollars exceeds the gross income of all the railroad and transportation companies in the United States and if we leave out New York and Pennsylvania, it would suffice to pay the public indebtedness of all other states in the Union, including that of all the counties, townships, school districts and cities within those States."

In a word, we have to do here with an interest, ranking third in the line of importance even from the mere view of dollars and cents, counting manufactures of all kinds first and agriculture second.

Now, while for education and for transportation, for agriculture and for manufacturing, the government has actively engaged in measures of promotion and has passed laws, looking to the preservation and increase of various forms of our natural wealth; while in the protection of game and fish, legislation has been active in almost every state; while by the establishment of a United States Fish Commission even the restocking of waters has been thought necessary; while millions of money have been expended for

clearing out the channels of streams and regulating their flow and protecting adjacent property against their floods, nothing has been done to protect the head-waters, where such damage originates; nothing has been done to protect the natural regulators of waterflow; nothing has been done to keep the mountain forests in continuous reproduction; nothing has been done to foster the recuperation of this important source of wealth.

The laying of a protective tariff has, if anything, hastened the depletion and encouraged the wasteful use of our forest supplies and beyond that no action worth naming and adequate to the importance of the subject has been attempted by the Federal government.

While this must be considered poor political economy, the absence of appreciation of what is the duty of government in this direction is still more marked in the case of the disposition and administration of that part of the public domain, which bears or bore a timbergrowth.

That this part of the public domain had any special value, needed any special protection like other valuable property, required any special administration to preserve its material substance and to secure the continued existence of the special climatic and hydrologic functions which the forest serves, such views of our forest property have hardly entered the minds of our representatives, if we judge by their legislative acts.

Even the latest bill which attempts a reform in the disposal of public lands, just passed by the House of Representatives, while it recognizes the greater material value of timbered property, fails entirely to comprehend its national and cultural value, and the discussions on this bill reveal strikingly this lack of comprehension.

Although we find, since the act of March 2, 1881, several feeble attempts to prevent the theft of timber from the public domain, these attempts were so feeble and inadequate, as to render them not only ridiculous, but like all laws which cannot be enforced for lack of executive organization, mischievous and dangerous to the morals of the community.

Passing over the history of this property in the past, we find that it has rapidly dwindled down, until to-day the United States Government owns but fifty to seventy million acres of forest area, roughly estimated—we have no means of approximating it more definitely.

This property is largely situated on the mountain ranges of the Pacific Coast and the Rocky Mountains.

It is constantly being diminished in extent by the land syndicates, who obtain possession of it by circumventing the law, with bribery and perjury, since no legitimate manner of obtaining the timber exists.

For lack of proper regulations and in the absence of proper administration, the rest is being robbed of its best timber partly with, partly without sanction of law, but in both cases in the most wasteful manner.

During the last seven years the value of timber reported as stolen amounted to $36,719,852; what portion this may be of the actual amount stolen may be inferred from the statement that over an area covering geographically not less than one million square miles twenty-seven agents only in the average were employed to ferret out and bring to justice the depre-

dators. The amounts actually recovered during this period, usually by compromise, amounted to $478,073, while the expense for this protective(!) service had cost $455,000.

So, after all, one of the commissioners of the Land Office could congratulate himself that it was a "paying" service.

With more and more urgency have the commissioners of the Land Office, the officers to whose care, under the Secretary of the Interior, this property is intrusted, insisted that under existing laws and conditions and with the deficient funds appropriated for the purpose not only is it impossible to protect the property of the people against theft and devastation, but the needs of the settler and the requirements of industrial pursuits being disregarded by the law, the users of timber are by necessity forced to become depredators if they wish to satisfy their needs.

In addition to the spoliations of its direct material value, the public timber domain has suffered untold damage by reckless, willful or careless firing and in every respect the management of this part of our national inheritance reflects discredit on our much-praised business capacity.

That this is not the fault of the Executive, but of the Legislative branch of our government which withholds the necessary funds for a proper administration of this trust, will appear from the constantly reiterated recommendations of the Secretary of the Interior, a few of which I cite as exhibiting the legal and moral aspect of existing conditions.

The Secretary of the Interior, in 1880, after devoting over six pages to the subject of forestry, says:

"I regret to say that in spite of the repeated recommendation of the passage of a law to facilitate the prevention of the wasteful devastation of the public timber lands, and to enable the government to dispose of timber to settlers and miners, as well as for legitimate mercantile purposes under such regulations as would prevent the indiscriminate and permanent destruction of our forests, almost all the legislation that has been had upon this subject consisted in acts relieving those who had committed the depredations in the past of their responsibility and protecting them against the legal consequences of their trespasses, etc."

And, in 1885, Secretary Lamar, repeating his recommendations in 1886 and 1887:

"The subject of the preservation of timber on the government lands has been suggested to Congress repeatedly in the reports of my predecessors. Perhaps its frequent repetition has rendered it commonplace, until it has come to be recognized as a part of routine report. Its importance justifies its repetition. That the timber is rapidly disappearing is an indisputable fact. Much is wasted and destroyed. Its effect on rainfall, the flow of our rivers, and the healthful character of climate are subjects worthy of consideration. Its importance and necessity for agricultural, domestic, and mechanical uses require no portrayal. Good government, while not forgetful of the present, should use some care for the future. Both on account of its present importance and its future necessity, this subject is worthy of your thought."

And further, speaking of the timber act of 1878:

"Its enactment was suggested, doubtless, by the fact that settlers in a new country, surrounded by woodland, could not and would not suffer in a rigorous climate for want of fuel and shelter; that the necessary industries of a frontier would not submit to the pinchings of a famine in the midst of abundance. But while it was necessary to recognize the inevitable, the recognition was not properly guarded, and waste and greedy speculation seem to have resulted from the law.

"Any timber for the uses named in the statute may be cut, under its provisions, by any resident of the Territory on any mineral lands of the Government, in the Territory of his residence, without compensation. Individual avarice and corporate greed, thus invited, with hasty eagerness, vie in accepting the bounty, and unless checked by wholesome modifications of the law, will soon cause all the mineral lands to be stripped of their forests. Railroads pass through many of the Territories; along their routes wealthy companies have been organized, mills erected, and the most valuable timber accessible is being rapidly cut off. That which is "every one's property is no one's care," and waste and extravagance are the natural consequence of negligent legislation."

The last report of the Commissioner of the Land Office (1887) contains a chapter illustrative of the manner in which a small minority has been for a long time defrauding the nation unchecked. Any citizen who feels himself a part of the great government "of the people, for the people, by the people" will do well to ponder over these pages of disgrace.

Under existing conditions not only is it made difficult for the resident population to supply itself with the needed lumber in an honest way, but the danger of doing so in contravention of the law entails an enormous, needless waste. Acres of timber are felled in anticipation of possible use, and rot on the ground, because their haulage may become too risky, or the depredator finds it difficult to dispose of the property, and so it is left to furnish food to the ever-recurring annual fires, which destroy not acres but miles of standing timber, and no legal disposition of the burnt timber may be made.

Such is the moral aspect of our present conditions in regard to the land laws and to the reasons for a change in our forest policy. The organic reasons are those which compel us to consider the forest cover of the mountains as of more importance than merely to supply material for the present.

That those who may cut timber legally on mineral lands, or homesteads, or timber entries on the Pacific slope, have no interest except to satisfy a present momentary need, and clear the land regardless of any consequences to future supply, or proper management, or forest conditions, utilizing only whatever part of the trees they may readily use or require, leaving the balance in the most wasteful manner on the ground, is attested by those acquainted with the manner of timber-cutting in those regions. Any sign of intelligent and systematic management which would insure a full utilization and continuity of the same is, of course, absent and not encouraged

by present regulations under the existing laws, and local supplies are waning in many parts. While in view of the needs of local supply for mining operations, especially in mines yielding low-grade ores, which cannot bear the burden of heavy charges for the importation of their timbering, this is an undesirable prospect, a much more serious danger is threatening the community at large in and around these mountain regions.

The climate is, in many parts of the region, not favorable to tree growth; at least not to the germination of seeds of coniferous trees, which form there the natural growth. These unfavorable conditions are, by the act of man made still more unfavorable. The wholesale clearing which is practised lays bare the thin soil to the influence of drying sun and wind; fires that sweep over the ground without hindrance destroy the thin mold and whatever seedlings may have been on it, and thus natural recuperation of the forest is made impossible, and any attempt at artificial reforesting is almost precluded. Barrenness and desolation are, as a rule, the result.

If, in view of so much graver consequences, it were permissible to allude to it, I would impress upon those who take a delight and a pride in the charm with which nature has endowed our country, vying with the finest scenery of Europe, that the beauty of the once verdant mountain sides is being ruthlessly and needlessly destroyed, and with such general equanimity is this devastation considered that we may soon substitute in our dictionaries the word "Americanism" for "vandalism."

What the graver consequences are can be readily understood by those who have studied the history of deforestation and forest devastation in southern France, Switzerland, Spain, Italy, and those far eastern countries which compare somewhat in climatic aspects with the region in question.

Not only is the forest cover of the mountain crests destroyed when it might have yielded continuous supplies, but at the same time agriculture in the valleys below is first endangered and then made impossible.

In a region which, like most of the plains of Idaho, Montana, Wyoming, Colorado, Utah, New Mexico, Arizona, and southern California, requires for agricultural purposes the aid of irrigation, regularity of water-supplies is all-important. This is being tampered with when the ground is laid bare on the mountain sides, allowing the rains to run off as from a roof and permitting the snows to melt and their waters to pour down in torrents at a time when more than enough water is on hand and the husbanding of the supplies for a later season is highly desirable.

Whatever may be the influence of the forest upon climatic conditions, its function as a regulator of hydrologic conditions is well understood and proved both by experience and experiment.

Other consequences, such as an increase of snow-slides and land-slides and the washing of débris into the valleys have begun to make themselves felt and it can only be a question of time when we must reach such a state of things as was brought upon the mountain districts of France, Switzerland, and the Tyrol, and which is too well known to be rehearsed again. I will only mention that after entire communities had been impoverished by the action of torrents due to deforestation the governments found it neces-

sary to interfere, or rather, interference coming too late, to assume or aid in the work of reforestation. Thus in France it was found that 788,000 acres needed to be restocked for reasons of public utility, besides the securing of 1,900 miles of torrents. One hundred and three thousand one hundred and thirty-eight acres of mountain land are reported already as put in condition by the Government at a cost of $4,865,750, outside of the cost of expropriations, etc. To this must be added $1,116,643, which have been given to communities and private owners in aid of similar works, and a further expenditure of about $34,000,000 is expected to be necessary. Altogether it is estimated that $30,000,000 have been expended to correct the evils brought on by foolish disregard of nature's laws. For the year 1887, the appropriation for these purposes amounted to $794,000, the total appropriation for the forest department of France being in round numbers $5,000,000.

There has been lately a proposition brought forward to build reservoirs in the mountains for the purpose of water regulation. This same proposition was made in France, but it was soon abandoned as impracticable, costly and as liable to create new dangers, perhaps greater than the old ones, since the mountain reservoir might burst at any time and the embanked river was certain to rise above the surrounding plain to a dangerous level. The wiser plan of reforestation, therefore, was resorted to with results which now have proved the wisdom of that measure.

The public land commission in 1883, recommending necessary changes in existing land laws, says: "The timber lands should be sold. Will not private ownership, self-interest, best protect this class of lands?"

If the history of the countries just cited, if the forest lands in the older settled parts of our own country, have not shown that this is a fallacy, we may never expect to learn from experience.

While the existing system of espionage and police may be "unpopular and un-American," as it undoubtedly is, it exists, not because there is no other choice than sale, but because there are no adequate provisions made to satisfy the requirements of lumber for actual and commercial use, thus forcing the population to depredations. Settlers and consumers of wood cannot be expected to go to the woods and cut their sticks when wanted, as in the pioneer days. They must have an opportunity to supply their wants in a business manner, as they do in all other needs of civilized life; through the agency of a middle-man — in this case the lumberman or the saw-mill man — nor, with the absence of stability in our population, can more than a temporary or an ephemeral interest attach to forest property for the individual.

It is not the forest that is valuable and would appear worth its protection to the individual, but the timber which the forest yields. As soon as that is gone, the value and interest are gone for the individual. The interest which the community has in the forest is transcendent. The continuation, reproduction, and protection of the forest cover are of significance to the continued welfare of the community, especially in the mountain forests, and the mountain forests will therefore be in safer hands with the community at large, with the state.

Let it not be overlooked that the state is not only the representative of communal interests as against individual interests, but also of future interests as against those of the present; that the forest is a kind of trust, of which the usufruct belongs to the present, and that to draw upon its capital is a perversion of the trust and can only be excused by direst necessity. Every other civilized country has found by severe experiences that private interest is not sufficient to protect this class of lands; that state ownership or, what is more objectionable and less effective, state supervision of private forest lands is indispensable in those regions where the forest subserves other functions than that of mere material supply.

We have then this proposition:

The United States owns a large forest property; owing to its location in a comparatively sparsely timbered country, careful husbanding and reproduction is important for the development of the surrounding country; owing to its location on the mountain sides and tops, within a comparatively arid region, which is dependent for its agricultural development on irrigation, its continuity as forest is imperative for the regulation and equalization of the waterflow in the mountain streams; experience and reasoning from the natural habits of private owners teach, that the expectation of indirect and distant benefits rarely induces such private management, as is likely to curtail present profits, and that individual owners will not be influenced in the management of their holdings by the interests and the resulting benefits to the community at large. Hence it is proposed that the United States Government reserve from disposal by sale or other entry, the forest lands unfit for agricultural use which it owns, that it place them under a proper forest administration, which with due regard for the needs of timber supply to the surrounding country will insure the continuity and recuperation of the forest cover.

I have embodied my views as to the precise organization of such an administration in a Bill, which has been before Congress during its last session, but in the turmoil of politics, in which political economy plays but a small part, this measure could hardly expect serious consideration. And yet the time is not distant, when it may be too late to ask for such an administration, because there will be nothing to administer.

I now call the attention of the members of this section to the early action of this same Association with reference to the subject of forestry.

At the Portland meeting in 1873 the Association appointed a Committee upon Forestry, "to memorialize Congress and the several State legislatures upon the importance of promoting the cultivation of timber and the preservation of forests and to recommend proper legislation for securing these objects;" Mr. G. B. Emerson being one of the Committee.

The result of this action and of the exertions of your Committee was an appropriation by Congress in 1876 to the Commissioner of Agriculture for the purpose of making inquiries relating to forestry, out of which has grown the present Division of Forestry in the Department of Agriculture.

Your Committee reported progress at the meetings in 1874, 1878 and, lastly, in 1880, it submitted a memorial designed for the various state legislatures, and asked for its discharge. With this the active influence of

the Association in shaping a forest policy for the country seems to have ended.

Several states have from their own initiative or as a result of individual and associated effort begun to give serious consideration to at least the management of their remaining state forest property, notably New York, California and Georgia. Not so the United States.

I, therefore, upon the grounds laid down in this paper, call upon this Association, through whose influence the only action for the promotion of forestry by the general government was secured, to further exert its influence in securing the more needful and more important action necessary to effect the reservation and administration of the remaining timber domain.

THE SCIENCE OF ECONOMIC ENGINEERING. By CHARLES S. HILL, Washington, D. C.

[ABSTRACT.]

EMBRACING the economic principles of developing, viz.:

1. Railroads.
2. Coast Defences.
3. Harbor Dependencies.
4. River Improvements.
5. Canals, Sluices of Irrigation, etc.
6. Public Education.
7. Shipbuilding.

Epitome: These economics constitute to the greatest degree, both civil and mechanical development of science, to constant employment of labor and to the grandest future of our nation in power, peace and prosperity.

These require wise economic legislation.

The development thereof is the only unification of capital and labor.

Arguments based upon the principles and precepts of Jefferson as a naturalist and political economist.

AN ERROR IN OUR NATIONAL DIETARY. By Prof. W. O. ATWATER, Middletown, Conn.

[ABSTRACT.]

EXAMINATIONS of the dietaries of a considerable number of people, in different parts of the country, show that their daily food contains an amount of nutritive material much in excess of the actual demand for nourishment. This excess consists largely of fatty substances and comes with the meats, which are consumed in great quantities. The meats in our markets contain much larger proportions of fat than is commonly supposed, a fact explained by our agricultural conditions and by physiological laws.

SHIPBUILDING AND SHIPPING AS A NATIONAL ECONOMIC. By CHARLES S. HILL, Washington, D. C.

[ABSTRACT.]

THE prestige, decline and prospect.

Their importance in utilization of labor, in developing a Naval Reserve and in securing a home circulation of currency.

The want of economic national legislation to revive these industries or their irrevocable disappearance as American interests.

The national dependence consequent.

THE INTERNATIONAL STATISTICAL INSTITUTE. By J. R. DODGE, Washington, D. C.

[ABSTRACT.]

THE recent biennial session of this Institute in Rome, Italy.

Its work and propositions.

The pressing need of a system of international comparison.

The future purposes of the Institute.

ECONOMIC VALUE OF BINARY ARITHMETIC. By HENRY FARQUHAR, U. S. Coast Survey, Washington, D. C.

[ABSTRACT.]

THIS paper aims to show (1) why this arithmetic, in the notation used by Leibnitz, is unavailable in practice; (2) how that disadvantage can be avoided by an improved form of numerical symbols; (3) what evidence has been given by actual experiment of labor-saving by the use of the binary scale; (4) to what degree and why the binary scale may be considered the natural one.

SUGGESTIONS FOR LEGISLATION ON THE CURRENCY. By EDWARD H. AMMIDOWN, New York, N. Y.

IN all considerations of the use of silver money, a distinction must be plainly drawn between the intrinsic and the nominal (or legal) values of gold and silver coins. Their nominal values may, under favorable circumstances, be maintained at a uniform ratio by legislation. Their intrinsic

values, depending upon the demand and supply of the precious metals, can never be permanent. And, as the conditions which determine the intrinsic values of gold and silver cannot be predicted, it is impossible to establish a ratio at which their intrinsic values can be maintained.

In the United States from 1790 to 1835, the intrinsic value of the silver dollar, as measured in gold, was less than its nominal value, and although the difference was only about five per cent, gold coins gradually disappeared. From 1835 to 1873, the position was reversed, and although the intrinsic value of the gold dollar, as measured in silver, was never more than five per cent below its nominal value, the silver dollar became a curiosity for collectors of rare coins. Since 1873, the intrinsic value of the silver dollar, as measured in gold, has again fallen below its nominal value, and gold coin, except as it appears in bank reserves or in the vaults of the treasury, is no longer an important part of the national currency.

In view of the great divergence which has taken place during the past ten years between the intrinsic value of silver, as measured by gold, and its nominal value in coin, it is commonly asserted that silver has declined in value; but it is also claimed by many that gold has increased in value; and with apparently good reasons, inasmuch as the world's annual product of gold has diminished during the past twenty years more than thirty per cent or from twenty-six millions to eighteen millions pounds sterling and its use in the arts and manufactures, together with its absorption in Asia, nearly equal at the present time its annual production.

Silver, on the contrary, has, as respects annual production, kept pace with the natural increase of the demand for it in the arts and for money, which has attended the growth of the world's civilization and trade. During the past ten years the product has increased about twenty per cent, or from seventeen millions to twenty-one millions pounds sterling per annum.

The effect of the diminishing production of gold upon its value has doubtless been largely augmented by the efforts of the European governments to substitute it for silver in the currency of their respective countries, thereby increasing the demand for gold concurrently with a rapidly decreasing supply.

The effect of these efforts upon the value of silver, lessening the demand concurrently with an increasing supply, must also have contributed not a little to the great change in the relative values of the two metals which is now disturbing the world's trade and everywhere provoking cries for relief.

How far the action of the United States, as a buyer of silver, during the past ten years to the extent of nearly one-third of the world's production, has neutralized European legislation adverse to silver, it is impossible to determine. But it is reasonable to believe that it has prevented a greater divergence of values between the precious metals and contributed in an important degree to maintain stability in the commercial relations of nations and individuals.

In view of the fluctuations of value in the past and of the certainty that

they will continue in the future, it is evident that the concurrent use of both gold and silver money, at constant relative values, can only be maintained by arbitrary law, prescribing the ratio at which they shall be interchanged, and at which they shall be received in payment of debts.

In the absence of such law coins of inferior intrinsic value could not be kept in circulation. But if, under the operation of law, coins of inferior intrinsic value are unduly forced into circulation, all coins of superior intrinsic value will be gradually absorbed by bankers and dealers in money; or, if the balance of trade is unfavorable, will be exported. Foreigners estimate our coin mainly at its intrinsic value, and elect payment of debts in that kind which has the highest metallic value, as compared with its nominal value. No apprehension need however be felt in regard to the export of either of the precious metals, whatever be their relative values, so long as a balance of trade in our favor is maintained. During the six years, 1880 to 1886, with favorable trade balances, this country has steadily continued to draw gold from Europe, and to export silver, in spite of the superior intrinsic value of the former. The net amount of gold received during that time is about $120,000,000, while the net amount of silver exported is about $60,000,000. The Treasury reports show that silver has been exported chiefly to those countries which use it most extensively for coin and in the arts, while the imports of gold have been from those countries with which our trade balances are finally adjusted. Silver has been exported as a metal for conversion to the uses of foreign nations. Gold has been imported in settlement of debt to this nation.

But while bi-metalism is possible only by force of law, no law can maintain it in the presence of permanently adverse trade-balances, and it is a problem for statesmen to determine to what limit, even if supported by favorable trade-balances, the force of law will, under given conditions, ensure the concurrent use of both kinds of coin. It is also their duty, if bi-metalism is desired, to guard against the transgression of that limit as well by encouraging the use of both, as by opposing all influences whether local or national tending to discredit either.

It is not essential nor desirable that the relative metallic values of gold and silver coins should be changed to correspond with the fluctuations of the market value of bullion, in order to secure their constant concurrent use as money. Variations in the production or use of either metal would speedily derange any ratio for the value of coins based upon the relative value of the metals. There are indications at the present time in the discovery of gold in South America and in the growing demand in Europe for more silver money, that the price of silver as measured by gold will be higher and that the nominal values of coins of the two metals will again approximate to their intrinsic values. The stability of our coinage will, therefore, be more surely maintained by avoiding any change in its composition.

The objections which are often made to the purchase of bullion for coinage, and the claim that coinage should be free, seem rather to be founded upon former usage than upon any valid reason. When, for long periods,

the relative values of gold and silver continued nearly uniform, and the influences affecting their values operated slowly and insensibly, free coinage was neither unreasonable nor prejudicial. It was a natural consequence of the assumed right of governments to the exclusive control of the coinage.

But, at this day, when the fluctuations of values of the precious metals are great and the conditions affecting their values are promptly known, no nation can justly permit free coinage of either metal or allow individuals to profit by the difference, which may at any time prevail, between the higher legal value of coin and its lower metallic value. It is certain that individuals will not turn metal into a coin at a loss. Is there any good reason why the government should not reap the profit of coinage for the benefit of the people at whose expense the mint is sustained?

It is sometimes urged that the purchase of bullion for coinage involves an increase of taxation, but it is evident that this cannot be true unless it produces an excess of idle money in the treasury, and only to the extent of the loss of interest on this excess. A large fund must be constantly at hand to meet current expenses. If a larger sum than is required for this purpose is at times accumulated, any loss of interest upon it will be materially, if not wholly, offset by the profit accruing from the purchase of bullion.

It may be fairly claimed that such purchases would be generally confined to that metal which, for the time, might be of lower intrinsic value as compared with its nominal value. The advantage of this course in maintaining the equilibrium of value between the two metals, as well as in profit to the government, is obvious.

It is scarcely necessary at this day to contend for the maintenance of bi metalism in the United States. The American people with almost instinctive regard for their own welfare, have determined that question. But it is not irrelevant to point out that it is for the advantage of the whole world that silver be continued in general use as money, on a parity with gold, and by force of law, to the largest practicable extent.

It is admitted that the scarcity of money involves the depression of the value of all other property and universal distress. However much individual or national credits, in the form of checks, bank bills, or paper money, may take the place of coin in business transactions, silver and gold money are as essential as ever and in a constantly increasing degree, for the convenience of mankind, as a basis and measure of values, for which no substitute has ever been, or, have we reason to think, ever will be found.

The historian, Alison, traces the decline of the Roman empire (among other causes) to the diminishing product of the mines of Spain. On the other hand, he attributes the vigorous advance of modern civilization in the sixteenth century, in no small degree, to the vast addition to the world's stock of gold and silver in consequence of the discovery of America. There are not wanting men of the highest intelligence on this subject who distinctly trace the prosperity of the whole world during the fifth and sixth decades of this century to the gold and silver products of

America and Australia. And eminent authorities now attribute the distress prevailing among all nations without exception—(happily less in the United States than elsewhere, due to our control of the precious metals of the world by means of our credit trade-balance)—to the gradual and constant diminution of the world's production of gold during the past twenty years. Whatever credence may be given to these opinions — and it is believed that they have not been successfully controverted—it is certainly the part of wisdom and prudence to prevent the demonetization of either of the precious metals and to afford every practicable facility for extending the use of both silver and gold money in proportion as the expanding business of the world may demand or justify it.

The power has been given in the Constitution and thereby the duty imposed upon the Government of the United States, to coin money and regulate the value thereof. President Jackson, in his message of Dec. 6, 1836, says, "It was the purpose of the Convention to establish a currency consisting of the precious metals." Daniel Webster said in a speech quoted by George Bancroft in his recent "Plea for the Constitution, etc.," "There can be no legal tender in this country under the authority of the Government but gold and silver."

Although, under the stress of civil war and justified by the supreme law of self-preservation, paper was by authority of government made legal tender, it may be confidently asserted that at no distant day the emergency will be considered the only ground for its justification. The decision of the supreme court which declares that "*it is consistent with the letter and the spirit of the constitution*" will then be reversed, and the judgment of the great framers and early expounders of that instrument, as expressed by Webster, be irrevocably confirmed.

No consideration of the coinage of silver or of the extent to which it may form part of the money of the country will be complete which does not contemplate the ultimate withdrawal of legal tender notes and the restoration of gold and silver money to its rightful place as the only constitutional legal tender, and the basis for all substitutes for money, such as certificates of deposit and bank notes, which have been invented and become necessary for the convenience of business.

The withdrawal of the legal tender notes would liberate the reserve fund of $100,000,000, now held by the treasury to protect the pledge of the government to redeem the legal tender notes in coin on demand. The loss incurred by the redemption of the notes would be the interest on the amount of the notes in excess of the reserve held to protect them. It may be estimated at three per cent on $250,000,000 or $7,500,000 per annum. For this comparatively insignificant sum the country would gain the advantage of an absolutely sound metallic legal-tender currency—a safe retreat from a doubtful construction if not a dangerous violation of the constitution — the substitution in our currency of a large amount of coin of international value, for paper of no value—a broader and more substantial basis for the business of the country and therefore greater uniformity in the supply of money and in the rate of interest, with a proportionate relief

from convulsive depressions in the value of property and from disasters in business.

The more extensive use of gold and silver money, which would follow the withdrawal of the legal tender notes, would be still further promoted by any diminution of the issues of national bank notes. It seems to be conceded that under the existing requirement of a deposit of United States bonds to secure the holder of bank notes, the declining rate of interest on the bonds is gradually rendering bank circulation unprofitable except during brief periods of prevailing high rates of interest.

The tendency of the money markets of the world, with increasing facilities for transferring money from one country to another, indicates for the future more uniform and generally lower rates of interest in all important commercial centres.

It is therefore probable that to some extent the use of gold and silver will be extended by the withdrawal of bank circulation. But it is not desirable that this should take place to any important degree. Banks for local issues of currency have become thoroughly incorporated into our financial system, and perform the indispensable function of regulating the expansion and contraction of the currency in conformity with the requirements of trade. This important function could not possibly be so well performed in any other way, and certainly no one could wish to see it transferred to the control of the government.

The use and circulation of silver money might be considerably augmented by paying the United States bonds and the interest as it accrues thereon in silver as well as gold coin. It cannot be successfully maintained that the outstanding bonds and the interest thereon may not rightfully be paid in either gold or silver coin or certificates, and so long as silver coin is kept upon a par with gold coin interchangeable one with the other in the purchase of commodities and the payment of debts, the value of the bonds and the esteem in which they are held for investments, would not thereby be necessarily depreciated either at home or abroad. On the other hand, the concurrent use of both gold and silver coin in payment of the bonded debt and interest, at the convenience of the government, would tend to make the intrinsic value of the two metals correspond more nearly to their nominal values in coin and partially counteract the effect of the demonetization of silver in Europe. One of the consequences of European legislation adverse to silver money, with the collateral larger demand for gold, has been to augment our national debt and the interest payment, if they are required to be paid in gold, to whatever extent it may be found that gold has appreciated during the last twenty years. Some conception of the degree of the appreciation of gold may be formed by comparing its purchasing value in commodities in 1875 and in 1885, although not all the decline in prices which will be found can justly be ascribed to any one cause exclusively. No inconsiderable part of it is due to increased facilities for transportation and improved methods of manufacture. To continue to pay our bonded debt and the interest thereon in gold or not to pay a large proportion of them hereafter in silver will not only close a

possible use for silver but strengthen the hands of those governments which have dishonored it, contribute to the support of gold mono-metalism, and thereby augment the burden of our national debt.

It may be said that silver money paid out on account of the national debt would be immediately returned through the custom house and little increased circulation be thereby obtained. But to this it may be replied that silver money paid in this way will remain in circulation as long as if paid out in any other way; and as long as any other kind of money would remain in circulation; the length of time will be determined not by the quality of the coin so long as parity is maintained, but by the prevailing requirements of business. All kinds of money including not only gold and silver, but their substitutes, national notes, bank bills, certificates, both gold and silver, in times of stagnation in business, are accumulated in the treasury and in public and private banks, lying idle and unproductive, because the diminished business of the country cannot use them profitably.

It is therefore no objection to the use of silver money or to the law which provides for coining it, that a large amount of it lies in the vaults of the treasury, or that it is impossible to keep in circulation all that has been coined.

The issue of certificates, limited by law to actual deposits of silver coin, is one of the least objectionable and most effective means to promote the use of silver money. It has the advantage of allowing the use of large sums without burden and the further advantage of saving the loss by abrasion which follows the use of the coin itself. It also gives to the government the benefit arising from the loss of certificates, which will inevitably amount to a considerable sum, and probably sufficient to cover the cost of storing the coin and issuing the certificates.

To what extent silver money may be issued, either in coin or by certificates of deposit, consistently with the concurrent use of gold coin, can only be determined by experience.

The existing conditions governing the solution of this problem have never prevailed before, and they are changing from day to day. Among them may be considered the volume of the world's business, its activity, the facilities and methods of local and international intercourse, the contrivances for the substitution of credits for money, the action of foreign governments, the changing customs of trade in Asiatic countries, the increasing demand for silver in those countries as their population and trade increase, and the growing home demand which would follow the abandonment of paper legal-tender, the reduction of bank issues, and the use of silver coin or certificates in payment of the national debt.

The policy of the nation on this question is therefore tentative, involving in its successful management an intelligent study of all the conditions which control it, and a broad and liberal view of our national interests.

The experience of other countries justifies the conclusion that a much larger amount of silver money may be used advantageously in the United States than is now used. Not only France, which employs more than twice as much silver money as this country, but Germany, which is preëminent-

ly hostile to silver, and other important nations furnish evidence that the amount of silver money in use here is far below the capacity of our people to employ with advantage. The rapid growth of our population also, of necessity, demands an increasing amount of money in circulation and especially that kind of money which passes from hand to hand in small sums. This is peculiarly the province of silver money. The facilities offered by modern suustitutes for money, such as checks and bills of exchange, serve the needs of large business transactions, and of the great bulk in value of the exchanges of the country. But they are not available in the daily transactions of the wage earners and of those, numerically the greater part of the people, who keep no bank accounts.

The field for the use of silver money is capable of great enlargement as population increases, and a country like ours, covering a vast area and sparsely settled, requires a larger per capita amount of silver money in circulation than smaller and more thickly settled countries where the people have readier access to the conveniences of modern commercial life.

It would appear, therefore, that financial legislation in the United States should aim to increase the demand and the use for gold and silver money throughout the country and to expand its volume in proportion to the growth of population and business. It should also encourage the free issue of national bank currency under similar rules to those which now prevail to secure the holders, and facilitate the easy and rapid expansion and contraction of its volume in harmony with the fluctuating requirements of trade.

Such a policy, intelligently and persistently followed, would give to the country a broad, substantial basis of metallic legal tender for its business transactions, and through the national bank currency furnish the means to maintain easily that equilibrium between the demand and the supply of money which is essential to continuous national prosperity.

THE PARITY OF MONEYS AS REGARDED BY ADAM SMITH, RICARDO AND MILL. By S. DANA HORTON.

[ABSTRACT.]

THIS paper was prepared in May, 1888, as an open letter, answering a question of a member of the English Royal Commission on Gold and Silver.

This Commission consists of twelve members, men of great distinction and competence, including prominent members of either House of Parliament, and has been engaged for two years in an inquiry into the whole matter of gold and silver money, the evils of the present situation, whether affecting England or India, and the possible remedies for these evils. The principal proposition for permanent amendment of the present disturbed monetary situation of the world is that of a triple or quadruple alliance of the great monetary powers, to assure equality before the law to the two

money-metals. This remedy was first projected in Europe and America, in 1876, when the great breach of parity between silver and gold occurred. A treatise of that year "Silver and Gold" (R. Clarke & Co., Cincinnati), by the author of this paper, established in his view the conclusion that it was the interest of each nation to promote such a mintage union, and that it was in the power of the United States to assist materially in bringing about its establishment. This policy was in the end adopted by Congress. Free coinage of silver was defeated by the Alison amendment of the original "Bland Bill" and the second section of the act of February 28, 1878, invited the nations to a conference on the subject. Subsequent acts reaffirmed this attitude. The history of the monetary conferences of 1878 and 1881 is familiar. Failing of immediate success in one sense, they exerted, and still exert, influence as a means of educating the nations to work together. They remain a power, also, through the literature they called into existence, including their reports, repositories of knowledge, which have been gratuitously distributed throughout the world.

In Europe the issues connected with the proposal of the United States have been the object of thoughtful study, and the occasion of a widespread revision of economic doctrine in which earnest representatives of the policy of the conferences have taken an important part.

The present paper is the latest of a close succession of works from the same author bearing upon this question. It deals with the central point of interest and of debate, which unite or engage all who think or speak of the silver question. Indeed, the parity of silver and gold makes up in one sense the sum of the whole matter. Monetary controversy since the panic of 1873 is, in fact, but a story of parity, a question of parity once enjoyed, and of its loss—about 1873. When is it to be regained? According to the delegates of the United States (of 1878 and 1881), it is to be regained when England shall be converted to see her own interest, and join the other nations in restoring it. When this occurs the number of great powers required to make the alliance for intermetallic parity will have been made up.

It thus appears that the question addressed to Mr. Horton provokes a hearing of argument on the main issue, before a court of high jurisdiction.

Is it clear that the power of government goes to the extent of enabling them to establish a stable ratio between silver and gold?

In his affirmative answer, the writer puts aside, as trivial and visionary, reasoning which the public has been wont to hear reiterated in support of the negative. Much of this reasoning, it appears, grows out of confusion between the economic and the political sides of the question. His affirmative applies to the question, as an economic question, and is stated in terms of the law of supply and demand. Parity, he says, is the inevitable result of a wise regulation of demand operating upon a limited supply. The supply of gold and silver is limited by their nature; what is needed are appropriate laws of nations to regulate their relative demand; that is to say, laws which give equality to the metals in their character as money, equality of legal privilege. Let the chief nations—including England, under the

advice of the royal commission — adopt these laws and the thing is done.

The only directly opposing arguments, to which the writer gives attention, are those which tend to "conjure up in some minds a mirage about the limits of supply." These arguments, he avers, belong to the order of gratuitous prophecies. The annual increment of the stock of silver and gold *always has been* a minute fraction of the stock already in men's hands: and there is no ground for supposing it *will be* anything more.

He then approaches his main theme which relates to the point of clearness. "How does it come to pass that doubt can exist on a point so simple and so clear as that parity can be so produced and maintained."

The doubt has existed in great force, and still exists in some measure. Why is this? There must be some cause at work to blind the minds of the learned, so that evidence and argument, such as that already adduced, are not appreciated.

It is all the more strange that doubt should exist when we observe its habitat. England, for example, is the scene of a series of remarkable parities. Ample instances of limited supply and of legal regulation of demand are in sight in England. Parity extends from bronze pennies to silver shillings, gold sovereigns, small notes, large notes and bullion. Nowhere is there a breach of the parity. And yet the doubt exists about the possibility of parity between silver and gold in spite of the fact that the very basis of discussing that possibility is a proposed legal regulation of demand, which shall be coëxtensive with the larger field in which the money metals are used.

What is the source of this doubt? It must be traced in the teaching of the leaders of economic thought; there must be a fashion of incorrect thinking about money and monetary laws and about the demand they regulate. Hence the review of the ideas of Adam Smith, Ricardo and Mill about parity.

They wrote while silver and gold were equally money, while phenomenal intermetallic parity was maintained by equalizing laws. But no effort had been made to ostracize one of the metals, and hence they were not imperatively summoned to investigate the cause of this parity. They omitted to investigate the question. They took silver and gold for what they were worth, without curiously asking the why and wherefore. Hence certain flaws in their teaching. In their action as political advisers about money, they were in a general way well disposed to both metals. But in their doctrine, in the monetary department of their systems of economic knowledge, defects appear. There is a lack of cohesion or junction between parts. The principles of monetary law, the nature of the institution of money, are slighted; and it is from this standpoint alone that the phenomenon of parity can be understood.

These defects are a natural outgrowth of circumstances. Modern economy arose as a reaction against an earlier system of economic learning, which is known since Adam Smith's day as the Mercantile or Commercial System. The central idea of this system was that national wealth consisted in national money; the important thing was, not the capital and the labor, not the savings and the skill of the citizens, but the cash they held.

This being the ancient view, it appears that gold and silver were, so to speak, in the very citadel of that doctrine of state interference, which Smith and his successors sought to overthrow and destroy. By a natural exaggeration all monetary law was drawn into the struggle, and a certain neglect of it, or even antipathy for it, was generated, which extended itself to the present day. In earlier days little practical harm came from this neglect; the thinker's good sense being a safeguard against the error of his theory. The harm came when disciples proceeded to build their schemes of governmental action upon the oversights of the masters.

It is this process which the writer seeks to expose and to check.

In a critical review of representative passages(of the works, especially, of Ricardo and Mill)the writer presents evidence of the defects thus pointed out. These evidences are brought in contrast with what he has from time to time advanced as the main generalization upon which amendment of the doctrine of the older economist must be based. The tendency in the past has been to dwell exclusively upon one phase of money, namely Voluntary Payment, while what he names the Institution of Obligatory Payment has to an important extent been ignored. Yet the latter, he avers,is as it is always has been, a vital factor in civilization; the power of the state to establish and regulate money is a power always essential to the life of the state. It is because they lacked this legitimate foundation of principle that the accredited systems of knowledge about money, as set forth in economic treatises, have been largely infected with the unpractical or the visionary. They are, as it were, honeycombed with metaphors.

Witness the popularity of the fallacy "Trade in Barter," or of the deceptive analogy between the pound sterling and the pound Troy or the imperial gallon.

This flaw in economic theory has tended to incapacitate the mind from accurately weighing the evidence and argument which establish the certainty that parity of the metals can be maintained by concurrent laws of nations. But for the little that remains of this unpractical and visionary element among the representatives of science the economic question is everywhere settled.

The political question remains. But it must no longer wear the robes of science; it must appear in its proper character as a political question.

Will the leading nations of Europe pass the laws, the principle of which the United States proposed for their consideration in 1878 and 1881? Upon this question the world awaits the action of the British Royal Commission on Gold and Silver, and of the British Parliament.

THE FOOD SUPPLY OF THE FUTURE AND THE DOCTRINE OF MALTHUS. By Prof. W. O. ATWATER, Middletown, Conn.

[ABSTRACT.]

THE current ideas, area and soil production are based upon agricultural experience. Modern science is showing that the fertility of the soil as ordinarily tilled is not the necessary limit of its capacity to produce plants.

This latter is really conditioned upon the supply of plant food and of water which reduces itself to the questions of the supply of energy for the transport of plant food and water and that of nitrogen as plant food. The indications are that with increased density of population there will come increased utilization of energy and of atmospheric nitrogen and that the production of plant food will be adequate to the demands of the race for the indefinite future.

THE AGRICULTURAL SURPLUS. By J. R. DODGE, Washington, D. C.

[ABSTRACT].

A DISTINGUISHING peculiarity of the United States is that it is a country of surplus. It has a surplus of productive resources in soil and mine, in invention and enterprise, in self-reliance and self-assertion. It has a surplus of national revenue without the consciousness of burden, and, of late, almost without a prospect of reduction. There is also a surplus of rural production in larger proportion to total volume than that of any country in the world. It is an element of strength and weakness, and, at the same time, is a subject for congratulation and regret. The congratulation is found in ability to relieve the deficiencies of needy nations while swelling the plethora of domestic wealth; the regret is for the tendency to over-production of certain crops, and its inevitable result. This is the reduction of prices for the benefit of the foreign purchaser without any advantage to the producer. Very few people know the extent of our net surplus in agriculture. Almost every one exaggerates it. An Ohio economist and politician once said that for a quarter of a century, one-fourth of the products of agricultural labor of the United States have been exported. It was an absurd and grotesque misunderstanding, and yet it has been repeated on the floors of Congress. The real proportion is about one-tenth. The error arose from two causes. The sum of production was taken from census tables, that give little else than the results of arable culture, not including the meats and many other articles, and it was only about sixty per cent of the true aggregate of production. But this blunder was not the only one. The value of the product is that of farm prices, while the exports are reckoned at export prices, the value at the seaboard, and not that of the farms. While the export value of this surplus exceeds $500,000,000, it was actually worth to the farmer less than $400,000,000. This is the size of the surplus. But there is another side of the question. There is a deficiency as well as a surplus. Suppose we take the latter from the former and find the net surplus; find what remains after paying for food and drinks which we obtain from other lands. The imports of 1886-7 amounted to $287,542,266. This was foreign value, to which must be added the cost of transportation, commissions, and profits to obtain the value on our shores, which must be something like $350,000,000.

Thus we have a surplus sufficient to pay for our deficiency, and little more. This is the net result of our boast of feeding the nations. We feed them just a little more than they feed us. The lesson we learn from these facts is that no nation can afford to have a deficiency of the raw products of agriculture, and as a rule, nations do not. There is one notable exception, and that is more apparent than real. Great Britain seems to have a large deficiency. Really, it is largely made good by shipments from her own colonies of the dividends of her own capital under the technical name of imports. Our agriculture, therefore, should seek to supply deficiencies rather than to swell surplus crops; to meet the present wants of domestic markets and create new wants by a greater variety of edible products, especially the fruits, and afterwards supply any deficiency of foreign nations that is practicable or possible.

THE NICARAGUA CANAL: ITS GENERAL FEATURES AND THE RESULTS ANTICIPATED FROM ITS CONSTRUCTION. By H. C. TAYLOR, Commander U. S. N., 34 Wall St., New York.

[ABSTRACT.]

IT is now a year since you were last addressed by us on the subject of the Nicaragua canal. Soon after the adjournment of your meeting of 1887 our company began the preparation and outfitting of a surveying expedition, the work of which, now complete, has been of the most careful and detailed character. I believe no large enterprise has ever been approached and prepared for with more careful attention or in more minute detail. While this expedition has been achieving excellent results in Nicaragua, preparation has been actively carried on in this country and Europe, and the interest aroused has been correspondingly great and widespread. There is, therefore, much that is new, both in the engineering features and in other aspects of the enterprise, which should, we believe, be brought before the association.

The late Mr. E. G. Squier, author of a well-known work on Nicaragua, adopted this line from Ovid for the title-page of his book, as a poetic characterization of the Nicaraguan route for an inter-oceanic canal:

Hic locus est gemini janua vasta maris— "This place is the vast gate of the twin sea." The image is a very happy one. Here the great *cordillera* which traverses the American continent from end to end sinks to its least elevation in the valley of the San Juan River. Between these mountains, bearing southeast from the Honduran frontier, and the coast range, skirting the west shore, of which Mr. Squier makes mention as "the undulating hills and ridges of land which intervene between the Lake and the Pacific,"— a basin is formed covering an area of 2600 square miles. This is the Lake of Nicaragua, one hundred miles long and forty wide, and, in places half a hundred fathoms deep. It drains a watershed of 8000 square

miles and has a mean flow of 1,272,158,600 cubic feet per day. From east to west along the sailing line of the proposed inter-oceanic canal it carries a channel deep enough to float the largest ships. Its level is a little more than one hundred feet above the ocean, and is subject to no material variations. Some of the streams falling into it from the north are of considerable size, and furnish a supply of water, in excess of evaporation, which could not be sensibly affected by drains for artificial purposes.

The San Juan River, flowing into the Caribbean Sea, is the only outlet of this great Lake. Together the lake and river will form more than three quarters of the total mileage of the canal. The utilization of natural basins and the creation of artificial ones, in the valleys of the San Francisco, the Tola, and the Deseado, will afford such additional free navigation as to leave little more than one-sixth of the total distance to be excavated in order that this "vast gate of the twin sea" may be thrown open to the ships and the commerce of the world.

The axis of the San Juan valley is nearly east and west. Across this depression and along the route of the canal the trade-winds blow almost throughout the year, favoring the entrance and exit of vessels, ventilating and drying the land, and lowering the temperature. The Atlantic slope of Nicaragua is densely wooded, which keeps the ground comparatively cool. Here the warm winds, having their temperature suddenly reduced, deposit their moisture. Over the lake basin, and the dryer country on the west, their temperature is raised, and instead of depositing they take up moisture. Hence the difference in climate between the east and west sides, — the latter being almost rainless from November to May. The sky is usually cloudless during this dry season, and trifling showers fall only at rare intervals.

The temperature of Nicaragua is equable. Observations of the thermometer for eight months from July to March showed a maximum of 87° and a minimum of 71°. The extreme variation is about 20° and the mean variation about 10°. The effect of the dry season has been likened to that of a northern winter, checking and destroying the ephemeral vegetation which is continually renewed where rains are constant. During more than six months that the recent canal survey was prosecuted, the entire force, numbering nearly 200 men, enjoyed remarkable immunity from disease of every kind. The most and the hardest work was on the eastern division, in the valley of the San Juan, or that portion of the line which might have excited apprehension on account of the heavy rainfall and the alluvial soil. A powerful modifying influence on this river is the circumstance that the great expanse of Lake Nicaragua makes rise and fall very gradual, and as the river is only an outlet of the lake, and receives no tributary of consequence above the San Carlos, it is not subject to floods and sudden changes of level, which might leave conditions favorable for the development of malaria.

The line of the proposed canal will lie entirely within the boundaries of Nicaragua and Costa Rica, and the people and governments of these Republics show a keen and active interest in the progress of the undertak-

ing. Guatemala, San Salvador and Honduras are also awakening to the fact that an enterprise of momentous import to their welfare has been inaugurated. Contractors experienced in the construction of public works in Central America have given the assurance that thousands of natives, accustomed to the climate and amenable to discipline, can be obtained for the construction of the canal when required. The recent experience of its engineers, with native laborers and men from the West Indies, has demonstrated that the labor problem will interpose no obstacles to the progress of the work.

Suitable timber, stone, lime, clay, sand and gravel for construction purposes abound along the line of the canal. With palm leaves for thatching, temporary shelter can be anywhere improvised in a short time. The cane huts of the natives are admirable for ventilation and for the coolness of their thatched roofs. More substantial structures can be erected according to the necessity for their permanency, at little other expense than that of handling the material. The country affords a supply of all necessary provisions,—beef, poultry, rice, beans, coffee, chocolate, sugar, fruits, vegetables, etc., so that little else than tools and machinery need to be imported for the work.

Such is the location of the projected ship canal from Greytown to Brito, and such the natural conditions along its immediate line of route. The engineers' estimates of the cost of construction include the electric lighting of the canal, the lighting and buoying of the harbors and the lake, and railroads from the lake to the Pacific and from Greytown to Ochoa, which, with lake and river, will form an immediate transisthmian connection and furnish facilities for communication and for transporting material during the period of construction.

The survey, the results of which are now in our possession, was not a preliminary nor an experimental one. It was an exhaustive scientific examination of a definite line, and the result has been referred to by the leading engineering journal of America as "a more accurate topographical map of that part of Nicaragua than exists to-day of the State of New York, excepting Coast Survey maps." All difficult portions were cross-sectioned at intervals of 100 feet, and all of the work was plotted on a scale of 400 feet to the inch, with 10 feet contours. As is well known, the Nicaragua route has been surveyed repeatedly in past years, so that the work of location just completed was performed under most favorable conditions for thoroughness and accuracy. The route itself was approved, during the presidency of General Grant, as the best and most practicable known route for a ship canal through the American Isthmus, by a Government Commission consisting of the Chief of Engineers of the United States Army, the Chief of the Bureau of Navigation, and the Superintendent of the U. S. Coast Survey, after a technical examination extending over several years, of the whole subject of inter-oceanic communication.

A contract was entered into in April, 1887, by the republic of Nicaragua with an association organized in New York, which secures to the latter exclusive right of way for the construction of the canal. A bill to incor-

porate the "Maritime Canal Company of Nicaragua," for the purpose of carrying that contract into effect passed the United States senate in February of this year, by a vote of thirty-eight ayes and fifteen noes. In reporting this bill favorably to the House of Representatives, the Committee on Commerce said: "The association asking Congress for a charter has secured from the Government of Nicaragua a most liberal concession, allowing a period of two years and a half within which to commence operations, a grant of a million acres of land, and immunity from taxation, imposts and duties for a period of ninety-nine years. . . . Your Committee is fully satisfied as to the financial standing of the Association."

If to-day the canal were open for the passage of ships, it is estimated that four-and-a-half million tons of traffic would be ready at once to avail of it. M. Paul Leroy-Beaulieu, the eminent French political economist and statistician, in an article published in February, 1888, placed at that figure the traffic between the Atlantic and Pacific oceans, within the zone of attraction of an American inter-oceanic waterway. Of this amount, statistics of the three great ports of San Francisco, Callao and Valparaiso, for the year 1885, alone showed a total of two-and-a-half million tons. Official figures of the Bureau of Statistics of the U. S. Treasury Department and harbor returns of the free ports of Panama and Colon, the termini of the trans-isthmian railway confirm M. Leroy-Beaulieu's estimate with remarkable exactness. The tonnage of the ports of Colon and Panama amounted in 1885 to 1,217,685 tons. For the year ending June 30, 1887, according to the U. S. Bureau of Statistics, the trade of the Pacific ports of the United States with home and foreign ports on the Atlantic was 1,025,557 tons. For the same period the trade between Atlantic ports of the United States and foreign countries west of Cape Horn was 752,585 tons. Statistics of Great Britain, Germany, France, Belgium, Holland, Italy, and Spain (those of Russia, Austria, Denmark, Norway and Sweden not being available), place the trade of those countries around Cape Horn in 1886 at 1,471,399 tons, and the trade of British Columbia with Europe in 1886 was 39,818 tons.

The total is 4,507,044 tons. The existing traffic within the zone of attraction of a canal through the American Isthmus may therefore safely be stated at this amount, whereof more than half is commerce of our own Atlantic and Pacific ports. The Suez Canal was opened in 1869, and its net tonnage in 1870 was 436,609 tons. In 1883 it was 5,775,861 tons, and receipts of a million dollars the first year were swollen to $13,702,413. It is true that the business of 1883 was the high-water mark for the Suez Canal and that its traffic has not maintained the previous rate of development. But that state of things results from facts and circumstances which increase the favorable outlook of the American canal when compared with its Egyptian forerunner. In 1883 the Suez traffic reached the limit of the canal's capacity, and began to be seriously delayed by the inadequate dimensions of the passage-way and the insufficient number of the turn-outs. The depth was twenty-six feet and the width on the bottom seventy-two feet, the surface width varying from 190 to 330 feet. Vexatious delays,

due to the grounding of vessels in turning the sharp curves and in going in and out of the sidings, interfered with the traffic so much and so often as ultimately to arrest its growth. The matter became so serious that a second canal was proposed by the British ship-owners, and the Suez Company was compelled to begin operations for deepening and widening the channel, and these are now in progress. The depth and the sectional area of all except a small portion of the Nicaragua canal will considerably exceed those of Suez, while in the lake, the San Juan river and the basins, its width will permit vessels to pass each other at their ordinary sea-going speed. Of the canal in excavation, all except a short stretch between the locks and in the rock cut, will have such width that vessels in transit can pass each other without inconvenience. The capacity of the canal will be at least twenty million tons per year, and could be doubled, if necessary, by duplicating the locks.

Another element which restricts the growth of the Suez Canal traffic is that it is limited to steamships. The physical conditions and erratic weather on a part of that route preclude its use by sailing vessels. Its development, therefore, is influenced in some measure by the fluctuations and exigencies of the business of ship owners and ship builders, for it is certain that a large part of the worlds' commerce is still carried by sailing ships. It is also true that in a measure the characteristics which close the Suez route to sailing ships render it undesirable for certain classes of steam-using vessels, navigating economically rather than with speed, or fitted only with auxiliary steam power. The Nicaragua Canal, on the other hand, will be admirably adapted for sailing ships and freight steamers. The trade-winds are most favorable to navigation in the Atlantic and the Caribbean Sea in the very latitude of its eastern entrance. Its western extremity lies in the same favoring belt, to the north of and entirely free from the calms and "doldrums" which vex the mariner in the region of the Panama isthmus.

The factor of the greatest importance, however, in influencing the growth of traffic, *via* Nicaragua, has no analogue at Suez. That is the certainty of the early and rapid peopling and building up of the territory served by the Nicaragua Canal,—especially the part lying within the United States. The commerce between Europe and the settlements, colonies and countries of India, Malaysia and China had reached a high state of development long before the Suez Canal was projected. It is doubtful whether that commerce is now increasing at all, and in any event its future growth must be at a very moderate pace. The natural development of our own Pacific states and territories, even at their present rate of progress, would soon furnish thousands of tons of traffic for the hundreds that now exist. The wheat, lumber, wool, fruit, hops, fish, oil, furs and mineral products of that virgin and imperial domain are just beginning to attract attention in the world of trade and finance. Its 100,000 square miles of the most available timber known to commerce, lying easily accessible from the ocean, have scarcely been looked at from the outside by the capitalists and merchants who before very long will be planting their

mills by hundreds in the hearts of those forests. The acreage and product of the wheat fields of Oregon and Washington have doubled in the past ten years, and there is enough vacant land there to permit the same phenomenon within the next decade.

Less than twenty years ago our first transcontinental railway was opened. Five great systems now rib the continent with their main lines and branches, and another twenty years will, in all probability call as many more into existence. Who shall measure the stimulating effect upon our commerce and that of our near neighbors to the southward, of the safe and sure progress of an inter-oceanic canal, in American hands and under American auspices, toward speedy completion? Who can estimate the quantity of the tonnage that will pass through that canal twenty years from the day of opening? The traffic which now exists assures a profit upon the cost of its construction, even were that cost estimated at double the amount of the engineers' figures. Half of that trade enriches our own producers and merchants, but they maintain it against foreign competition under overwhelming disadvantageous conditions, which the canal would largely neutralize and in some conspicuous instances entirely reverse.

A map of the world will show Liverpool and New York to be about the same distance from San Francisco by the way of Cape Horn. The actual mileage is, from New York 14,840, and from Liverpool 14,690 miles. The saving, by the canal, between places near its western extremity and our Atlantic Gulf ports, will be so great that it is difficult to understand how it could fail to result in turning the entire trade of those ports, which now goes to Europe, into our markets. Callao and Valparaiso, which will be, respectively, 3,000 and 4,000 miles from New Orleans, will be 6,461 and 7,448 miles from Liverpool by the shortest route, — the canal. It would seem that such conditions must necessarily result in breaking down the artificial trade routes and relations which now exist and turning the commerce of these neighboring countries into more convenient and natural channels.

The opening of the Nicaragua canal will mark a great epoch in commercial history and progress. New and better routes and connections will be at the service of North and South and Central America, Mexico, China, Japan, New Zealand, Australia and the Pacific Islands. Their development will be quickened and its tendency shaped by these new relations. Intercourse between distant parts of the world will be cheapened and facilitated. The two great branches of the Anglo-Saxon race will meet in friendly rivalry on the Pacific, and spread yet farther abroad the language which ultimately is destined to regulate the commercial transactions of the entire globe. Our own commerce must increase notably in extent and importance. Coasting traffic between the Atlantic and Pacific doubtless will expand to enormous proportions. The ports of the Atlantic and the Gulf will send their regular lines of vessels and their occasional traders up and down the Pacific coasts, just as they now send them into every harbor and river and lagoon of the Caribbean and the Gulf, exchanging our commodities for the products of the tropics, until ultimately the hold of Europe upon the

trade and commerce of Spanish America will be broken by the natural growth and influence of intercourse between the various countries of our own continent under most favorable conditions. Direct, speedy and inexpensive water transportation will put an end to the isolation which has so long retarded the development not only of the Spanish states of the west coast, but of our own magnificent territory bordering on the Pacific ocean.

Unless I greatly overestimate its bearing and effects, the forward stride about to be taken in the march of the world's commerce is to be a mighty one. When Byzantium was the glory of the Lower Empire, her merchants gathered the precious commodities of the East and sent them across the Mediterranean. Venice seized the inheritance, and for a long period maintained her position as the great entrepot of the eastern trade. After a fierce struggle, Genoa wrested from her the commercial supremacy and established herself as the western mart of this rich traffic. Still westward, from Genoa to Cadiz, and Lisbon, and Antwerp and Amsterdam, the great receiving and distributing centre shifted with the march of the ages, until finally the movement seemed to halt in England, and now for centuries London has been the commercial heart of the world and the controller of its exchanges. Nor is it strange that long preparation should be necessary for the next great westward stride, for it must cover a distance of 3,000 miles. It is this huge leap, well prepared for by the commercial strength and solidity of the lusty young nations of the Western continent, that is to be made by the opening of the Isthmian ship canal. The products that have hitherto gone westward to their markets will now go toward the east, while the manufactures of Christendom which have so largely gone east will now move westward. The step will indeed be a long one, and the revolution in the peaceful paths of trade throughout the world will be unique.

We of the United States can await this impending revolution with equanimity, for, much as it will help the world at large, it will most of all richly benefit the American continent.

NOTE.—This paper was in a good measure prepared by Mr. J. C. Hueston, of New York; based, in part, on suggestions given by Commander Taylor, but, in part, entirely original with Mr. Hueston.

RECENT NICARAGUA SHIP CANAL SURVEYS. By Civil Engineer R. E. PEARY, U. S. Navy, Washington, D. C.

[ABSTRACT.]

A YEAR ago I had the honor of addressing the Association upon this grand subject.

Seven months of the time intervening since then I have spent in Nicaragua with a large force of engineers and laborers, engaged in making surveys, the result of which I have the honor of presenting to you to-day.

These surveys, executed upon general plans formulated by Civil Engi-

neer A. G. Menocal, U. S. N., who can justly lay claim to being called the evolutionist of the Nicaragua Route, have given more than satisfactory results, and I am able to lay before you plans and profiles based upon information as complete and accurate as any upon which a work of similar magnitude was ever commenced.

These results are due first to the master mind who planned the work, and whose extensive previous experience in that country made every day's work of the recent expedition effective, and second to those fearless, hard-working engineers who in spite of obstacles and hardships of which you have no conception, cut their way through the tropical tangle until they knew the shape of every hill, the course of every stream.

The methods of work were as follows: the expedition being divided into parties and the work into sections, the locations of Mr. Menocal in 1880 in the western division, and of the government expedition of 1872–73, and Mr. Menocal in 1885, in the eastern, were taken as a base, and a main transit and level line run and bench marks established about every 1000 to 2000 feet. These benches were then checked. From this transit line, compass, chain, and aneroid, offsets were run from 1000 to 2000 feet on both sides, adjacent streams, valleys and hills reconnoitred; and the work plotted.

With this chart in hand the entire line was then gone over in the field by the engineer in charge accompanied by the chief of the section, and the location decided upon. The location was then run in and levelled, checking upon the benches of the preliminary line, and cross-sections run and levelled from 100 to 400 feet apart, along the main line as the topography demanded. Sometimes portions of this location were modified and re-run.

Streams were then surveyed and gauged, neighboring elevations beyond the limits of the canal taken with the aneroid, and the entire work plotted on a 400 ft. scale with 10 ft. contours.

The boring party then went over the line, boring on all summits and in all depressions, and penetrating to the level of the canal bottom unless rock was encountered above that level. Borings were also made on the sites of all locks, dams and embankments.

The work accomplished by the expedition is as follows:

In the western division two locations, one for a canal in excavation the entire distance from the Lake to the Pacific, and one for a canal with a basin at La Flor.

Survey of Brito Harbor, and of the Lake in the vicinity of the mouth of the Lajas.

In the eastern division — survey of the Lake from the mouth of the San Juan to deep water.

Re-survey of the Upper San Juan from Fort San Carlos to Castillo.

Two locations from the Ochoa Dam to Greytown; one surveyed by the government expedition of 1872–73, and known as the lower route; the other surveyed by the government expedition of 1885, and known as the upper route.

Re-survey of Greytown Harbor.

In all, the expedition cut, ran with transit, or compass, and levelled, 500 miles of lines, and ran 400 miles of soundings.

The first location in the western division, above mentioned, is practically the same as that of 1885, the only changes being such as would naturally be expected as the result of a final detailed survey, viz.: reduction in length from 17.27 to 17 miles; enlargement of the minimum radius of curve from 4000 feet to 4911 feet and reduction of total length of canal in curve.

In the second location the entire plan of the western section of the western division is changed as follows:

Three and one-half miles back from Brito, an earthen dam seventy-five feet high and 2,100 feet long on the crest, is thrown across the narrow gap in the coast hills through which the Rio Grande makes its exit to the Pacific. This dam will impound the drainage of the Rio Grande and Tola basins, and the water of the Lake, flowing through the summit cut; flood these valleys to the level of the Lake and form a basin 5.28 miles long, with a depth of water of from 30 feet to 70 feet. At the northern end of the dam a double lock of 85 feet lift effects the descent from this basin to the low coast land, and the canal then runs in a right line to the Port of Brito three miles distant, a second lock of 25 feet lift one mile from the double lock, dropping the canal to the sea level. This basin will be the harbor of the Pacific terminus and looking back from a steamer's deck as she emerges from the upper lock the port of Brito is visible just below, and beyond, the view of the blue expanse of the Pacific is unobstructed from north to south. From the eastern end of this basin to the lake, the two locations coincide.

The total distance from Lake to Pacific is not affected by this modification, but 5.28 miles of lake navigation are substituted for an equal length of canal; the length of actual canal is reduced to 11.72 miles; one lock is dispensed with; the problem of the disposition of the Rio Grande drainage is much simplified, and that of the Tola eliminated entirely.

The middle division, comprising the lake and river, remains unchanged.

The Ochoa dam may be constructed as proposed in 1885, viz.: a timber sheathed concrete monolith, or upon further study of the data obtained by the last surveys, it may be made an earthen dam and the overflow weir cut in the solid hills on the south side of the San Juan opposite the mouth of the Machado, thus removing the river current still farther from the entrance to the canal.

In the eastern division also, as already noted, there are two locations.

The one known as the upper route is identical with that of 1885 for a distance of four miles eastward from the Ochoa dam.

From this latter point to the Saltos de Elvira, a distance of eight miles, modifications have been made, though the general plan is unchanged.

The long embankment proposed in 1885, across the main valley of the San Francisco below the junction of the Chanchos, is replaced by five shorter ones about 1700 ft., 1200 ft., 1400 ft., 1700 ft., and 1200 ft. in length, respectively, across the valleys of the Chanchos, San Francisco, and tributaries, and there will be some 12000 ft. of secondary embankment at various

points along the crest of the impounding ridge, with a depth varying from five to thirty feet.

The sailing line is shifted farther north, and the flooded area in the San Francisco basin reduced.

The rock cut through the San Francisco-Deseado divide is practically unchanged.

The lift of lock No. 3 at the eastern end of this cut is, however, reduced from 53 to 35 feet. This change, in combination with a dam and a second lock of 40 feet lift farther down the Deseado Valley, gives a basin of 3.84 miles long and with a depth of water of from 25 feet to 40 feet, requiring excavation to an average depth of 2 to 3 feet along the upper half (1½ miles). The dam just mentioned is 750 feet long on the crest and 40 feet high, and is in a very favorable location, affording an excellent site for the forty feet lock in the hill which forms the south abutment of the dam, and an equally favorable site for the waste weir beyond the northern extremity of the dam.[1]

From lock No. 2 to lock No 1, a distance of 2.70 miles, the canal still follows the axis of the widening and gradually descending valley. In this reach the level of the water in the canal will be a few feet above the average valley floor level, and a low embankment across the valley at the site of lock No. 1 will keep this portion of the valley flooded sufficiently to largely decrease ship resistance and enable the surface drainage to be received directly into it and discharge over a weir at the dam. Lock No. 1 has a lift of 31 feet and from it to Greytown a distance of ten miles, the canal, at sea level, extends in a straight line across the flat coast lands of the San Juanillo and Lagoon regions.

By these modifications in the Deseado Basin, not only is the amount of excavation in this division largely reduced, but the free navigation is increased and the lateral drains the entire distance from lock No. 3 to lock No. 2, a distance of 6½ miles, are rendered unnecessary.

The total distance from ocean to ocean by this route is the same as before, 169.8 miles. Of this distance, however, only 28. 9 (instead of 40.3) miles are actual canal, the remaining 140.9 miles being open navigation through Lake Nicaragua, the Rio San Juan and the various basins. The length of the summit level has also been increased from 144 miles to 150 miles.

The lower route is identical with the upper as far as a point about 2½ miles east of the dam at Ochoa. At this point of divergence, either a single or double lock of 56 feet lift will lower the canal to within a few feet

[1] Since the above was written the locations of locks 3 and 2 have been changed as follows:

Lock No. 3 is now located on the site of No. 2 as above. Its lift is 45′. Lock No. 2 is located 2,570′ farther down the valley and its lift is 30′.

The dam across the Deseado at lock No. 3 will be 820′ long and 75′ high.

As a result of these changes the Deseado Basin will be four miles long with a depth varying from 25′ to 75′, and its surface will be 106′ above sea level instead of 71′. The length of the summit level will be increased from 150 miles to 154 miles.—R. E. P.

of the San Francisco Valley level, and then the canal extends across this valley behind low hills lying nearer the San Juan, its level kept up by a low embankment about 15 feet high, supplementing the hills; towards the bend of the San Juan, where the San Francisco hills come to the river. From this point it extends in a straight line to the Serapiqui hills 6½ miles lower down the river, being kept up through this section also by a low embankment on the river side. In these hills the second lock of 20 feet lift is located, thence the canal extends to the San Juanillo, still following the general course of the San Juan, and thence in a nearly direct line to Greytown, through the delta of the San Juan, the Silico Hills where the third and last lock of 30 ft. lift is located, and Silico lagoon. The additional length of this route over the upper is 6.6 miles and with the exception of the portion through Lake Silico (1.53 miles) it is all canal in excavation.

Though based upon the location of 1872–73 it differs from that in the following particulars:

It is 3½ miles shorter.

It has about one-half the number of curves.

It has no curve of less than 6,000 feet radius, and can be located with none less than 10,000 feet.

Its disadvantages, as compared with the upper route, are greater length and consequently greater cost of maintenance.

Thus we have at Nicaragua two perfectly practicable locations of about equal cost, either of which is far superior to any other route across the Isthmus: and when the day comes, as it surely will, when one canal cannot accommodate the traffic seeking it, then the other can be built and give one canal for eastward, and one for westward bound vessels.

I regret that the computations of the notes of the recent surveys have not as yet been completed, and I am consequently unable to give precise quantities and estimates. But the close correspondence between the measurements of former, and the last surveys, enables me to say in general terms, that quantities in sections where no modifications have been made will be changed little or none, while the total saving by modifications of plan will amount to $10,000,000 or $15,000,000.

The value of the 25% contingent estimate is also greatly enhanced by the closer determination in the recent surveys of all factors in the problem.

Probably in no similar undertaking have the conditions for the rapid and economical execution of the work been more exceptionally favorable.

In the western division that portion of the canal from Brito to the first lock, and possibly from the first lock to the double locks, can be dredged.

The Upper Rio Grande will furnish water for the removal of the surface earth by hydraulic mining, and later for running the rock drills in the divide cut. The deeper portions of the La Flor basin offer convenient and ample room for depositing all the material from the divide cut not used in the La Flor dam, and it will not be difficult to devise a method by which the water of the Lake can be made to give powerful assistance in the work of excavating and removing the material in the cut between the La Flor basin and the Lake.

In the eastern divide on the upper route exist similar conditions of water power available for removing surface earth hydraulically, and later for running air compressors for the drills. The deeper portions of the Chanchos and San Francisco valleys contain ample dumping ground for the spoils from the divide: and in removing these the engineer will have gravity with, instead of against him.

From Greytown to the first lock in the eastern section the excavation will be entirely dredging.

Probably not less than seventy-five per cent of the excavation between lock No. 1 and lock No. 2 also in the Deseado and San Francisco basins, and in the section from the latter basin to the dam, can by a proper sequence of work be excavated by dredges. On the lower route at least eighty-five per cent in length can be dredged, and the maximum haul of the earth and rock spoils on any portion of the remaining fifteen per cent will be about one-half mile.

The item of earth excavation, with all its varied plant of excavators, cars, locomotives, etc., its attendant expense of moving tracks and keeping tracks in order, and the difficulty of handling the material in rainy weather, is thus reduced to a minimum, and the excavation of the canal accomplished practically under the three great heads of Hydraulic Mining, Rock Excavation, Dredging, all three independent of drainage and rains, the great drawbacks to work on the Isthmus, and the work on the canal can be pushed forward without interruption night and day, year in and year out, until it is completed.

The numerous borings made have banished the bottomless swamps, the semi-liquid quicksands, and the numerous other subterranean bugbears which have been conjured up against the Nicaragua route, and have shown that in no portion of either location is there any trouble in regard to foundations.

In the wórst swamps encountered, the boring tools after sinking by their own weight for a distance of ten, or at a maximum fifteen feet, reached a stratum of firm red clay extending to bed rock. The sequence of strata everywhere except in the San Juanillo region, is almost without exception as follows: in the low bottoms, black mud or loam; then varying strata of blue or red or yellow clay or sand, or both, or all; then firm red clay; then, if the borings were carried deep enough, rock; on the hills, red clay and earth to bed rock. The borings show the even and unbroken character of the rock in the divide, and have also developed the fact that the earth covering of the bed rock averages deeper than was supposed in 1885.

In the valley of the San Juanillo, sand is found mixed with soft blue and yellow clays, and nearer the harbor sand alone. Borings at Greytown bar and in the harbor to the depth of forty feet below sea level, discovered no rock, only compact homogeneous sand, and on the upper route no rock was discovered within ten and a half miles of Greytown nor on the lower route between Greytown and the Silico Hills. The borings also show rock foundations for all the locks and the Ochoa dam.

LOCKS.

The locks in general dimensions and methods of construction remain the same as proposed in '85, viz.: 650 feet long and 80 feet wide.

The number, however, will be reduced from seven to six, and possibly five: the total lift will be somewhat differently distributed: a new feature is introduced in the shape of the double lock at the La Flor dam, and it is more than probable that improvements will be made by which the time of lockage will be reduced from forty-five minutes, as previously estimated, to thirty minutes. The gates proposed for the locks are sliding caissons for head gates and two tail gates, and rolling caissons for the other tail gates.

WATER SUPPLY.

The amount of water necessary for working the canal to its full capacity, viz.: forty-eight double lockages per twenty-four hours is 210,161,280 cu. ft. or less than one-fourth the minimum supply of the Lake alone (984,096,000 cu. ft.) without taking into consideration the flow of the several tributaries of the San Juan between the Lake and the Ochoa dam, and the San Francisco and its tributaries, which will more than compensate for all losses by evaporation in the flooded portion of the river, and the basins.

DIMENSIONS AND CAPACITY OF THE CANAL.

Some modifications of the cross-sections proposed in 1885 may be made, but for the present they may be assumed to be the same.

They will be ample to accommodate all the traffic that the locks can handle, and the capacity of the locks will be the measure of the capacity of the canal.

The maximum capacity of the canal was estimated in 1885 at 20,440,000 tons, based upon a time allowance of forty-five minutes for passing a lock, and the average net tonnage of the vessels using the Suez canal in 1883, viz.: 1747.

In 1886 the average tonnage at Suez was 1863 tons, and the constant tendency has been towards an increased average tonnage, which increase would have been much greater had it not been for the limitations imposed by the depth of only 26 feet at Suez and the insufficient facilities for vessels to pass each other.

With a depth of 30 feet at Nicaragua and considering the heavier class of traffic which would seek that canal, a conservative estimate would be that the average net tonnage of vessels using Nicaragua would almost immediately reach 2,000 tons or more.

With the probable improvements in the locks already noted, by which the time of lockage may be reduced to thirty minutes, the ultimate capacity of the canal will be forty-eight vessels per day of 2,000 tons each, or 35,040,000 tons annually.

CLIMATE.

Much has been said both for and against the climate of Nicaragua, but the experience of the recent expedition is worth volumes. Over forty engineers and assistants, not half a dozen of whom had ever been in tropical countries before, and some of whom were college graduates, who had never seen a day of really hard work or exposure, went to what is by common consent, the most unhealthy portion of Nicaragua, viz.: the region extending thirty miles back of Greytown, started at once into the woods, before the rainy season had closed, worked in the rain and swamps all day, and repeatedly slept on the ground at night, and yet in all the seven months that the expedition remained in the field not a man was lost, nor was there a single case of serious illness. More than that, every member of the party that has thus far returned, has come back in better condition than when he went away. I myself have repeatedly spent several successive days and nights, the former in traversing the lines of the various parties, the latter in sleeping as best I could uncovered and in wet clothes in my boat, while my crew who had slept during the day, paddled along the San Juan or up the San Francisco or Deseado, and at the end of the trip felt as well as when I started.

This was due, partly to the fact that no pains were spared to supply the expedition with the best of everything in the way of provisions, and partly to certain regulations which were rigidly insisted upon, but more to the climate and equable temperature, in which a sound man, with the simplest precautions, cannot be other than well.

In conclusion, I am happy to say that it is not now a question as it was a year ago,—shall we build the Nicaragua Canal? but we will build it.

The last year has seen a great verdict in its favor. In 1876, the great International Canal Congress at Paris, resolved that if a canal with locks were to be built, then the Nicaragua route was the best, but as a sea level canal was a *sine qua non*, and as such a canal was feasible at Panama, Panama was the only place for a canal. To-day, preparations are being made to put in locks on the Panama route, and not five or six as at Nicaragua, but ten, and the summit level of a canal which is said to be intended to accommodate the traffic of the world, is to be supplied with water by pumping.

This is the age of ship canals. Whatever sophisms may be put forth against them, ocean commerce recognizes their economy and utility, and in response to its demand they are being built and will continue to be built until every path of ocean commerce is reduced to its minimum length. European nations are building them for purely local traffic or from political considerations. Within the year, England has commenced upon the Manchester Ship Canal, and is to-day removing earth at the rate of 1,000,000 cubic yards per month. Russia has completed the Petersburgh and Cronstadt Canal, and is projecting another (Sea of Azof). Germany has commenced upon the North Baltic Canal. France is at work upon the Corinth. Suez and Amsterdam are doing an annually increasing business. But greatest project of them all, pledge of incalculable benefits to ocean traffic and

of commercial prestige to this country, is the Nicaragua Canal. It is the one grand link yet wanting to enable commerce to circle the globe without going beyond the limits of the tropical zone.

The gentleman who preceded me has covered the general bearings of the theme most eloquently, yet I cannot help trenching a bit upon his domain. The Nicaragua canal will bring every Atlantic port nearer every Pacific port; it will move the mouth of the Mississippi to the centre of the Pacific; it will give our western coast an impetus which will in a few years place it as far beyond what it is to-day as it is to-day beyond what it was when our statesmen were discussing the advisability of its purchase.

It will be a national stimulus to invention and with its great demand during construction, for men, machinery, materials and supplies, will offer a field for engineers, architects, machinists, contractors, business men, skilled labor of every kind, and a great market for supplies, which of itself alone will be of the greatest value to this country.

ON THE LIFE AND WORK OF E. B. ELLIOTT. By MRS. L. O. TALBOTT, Washington, D. C.

THE TRUE BASES FOR DEALINGS IN MILLS. By HENRY E. ALVORD, Agricultural College, Maryland.

THE MONEY HOARD IN THE UNITED STATES TREASURY. By EDWARD DANIELS, Washington, D. C.

OUR MONETARY SYSTEM. By EDWARD DANIELS, Washington, D. C.

THE HISTORY OF STATISTICS AND THEIR VALUE. By WILLIAM F. SWITZLER, Bureau of Statistics, Washington, D. C.

CREMATION: PAST AND PRESENT; WITH DESCRIPTION AND DIAGRAM OF THE BUFFALO CREMATION COMPANY'S FURNACE. By CYRUS K. REMINGTON, Buffalo, N. Y.

INDUSTRIAL TRAINING FOR VAGRANT CHILDREN AND IMMIGRANTS. By MRS. LAURA O. TALBOTT, Washington, D. C.

ABSTRACT OF THE PROCEEDINGS OF THE CLEVELAND MEETING OF THE ENTOMOLOGICAL CLUB OF THE A.A.A.S.

THE Club met as per announcements, Aug. 15, at 9 o'clock, A. M., the President, Mr. JOHN B. SMITH of Washington, in the chair. The Secretary, Prof. A. J. COOK, being unable to attend, Prof. HERBERT OSBORN was elected Secretary for the Cleveland meeting.

Sessions of the Club were held on Wednesday, Thursday and Friday during hours when Section F was not in session. The following members were present: John B. Smith, Washington, C. V. Riley, Washington, L. O. Howard, Washington, D. S. Kellicott, Buffalo, N. Y., O. S. Westcott and Mrs. O. S. Westcott, Chicago, F. M. Webster, Lafayette, Ind., C. J. S. Bethune, Port Hope, Ont., James Fletcher, Ottawa, Canada, J. Mackenzie, Toronto, S. H. Peabody, Champaign, Ill., E. A. Schwarz, Washington, D. A. Robertson, St. Paul, Minn., A. B. Mackay, Agricultural College, Miss., S. B. McMillan, Signal, Ohio, L. C. Wurtele and Miss Wurtele, Acton Vale, P. Q., Herbert Osborn, Ames, Iowa.

The annual address by the President, J. B. Smith, entitled Entomology and Entomological Collections in the United States, was delivered on Wednesday afternoon and subsequently discussed by Messrs. Riley, Howard, Fletcher, Webster and Osborn.

Mr. James Fletcher gave an interesting account of collections and entomological work in Canada.

A note On the Origin of the Wing in Aleurodes was presented by Prof. Herbert Osborn and illustrated with microscopic preparations.

Papers by Mr. Clarence M. Weed were presented and discussed as follows:

On the Parasites of the Honey-suckle Sphinx, *Hemaris diffinis* Boisd.

On the Hymenopterous Parasites of the Strawberry Leaf-roller, *Phoxopteris comptana* Frōl.

Professor Osborn presented a paper, On the Food Habits of the Thripidæ.[1]

Dr. D. S. Kellicott presented a note on *Hepialus argentio-maculatus*.

Prof. Herbert Osborn presented a note On the Occurrence of *Cicada rimosa* Say, in Iowa.

Prof. O. S. Westcott in a paper entitled "Entomological Memoranda" presented notes On Attraction of Carrion for Butterflies; the coincident occurrence of varieties *marcia* and *morpheus* of *Phyciodes tharos;* peculiar place of pupation for an Agrotis (?); occurrence of *Lachnosterna fusca* and *gibbosa*.

Mr. L. O. Howard remarked upon experiments with kerosene emulsion for underground larvæ.

[1]Published in full in *Insect Life* No. 5.

Mr. James Fletcher gave an account of an expedition to Nipigon north of Lake Superior and described methods of rearing butterflies from eggs.

Mr. E. A. Schwarz presented a paper entitled, The Insect Fauna of Semitropical Florida with special regard to the Coleoptera. This called forth a long discussion participated in by Messrs. Riley, Smith, Howard, Bethune, Fletcher, Peabody and Osborn.

The election of officers for the next annual meeting resulted as follows: *President*, JAMES FLETCHER, Ottawa, Canada; *Vice President*, L. O. HOWARD, Washington, D. C.; *Secretary*, D. S. KELLICOTT, Columbus, Ohio.

In accordance with a vote of the club the proceedings have been published in full in *Entomologica Americana* and all papers not otherwise indicated have been included in full in that report, which will be found in Nos. 6-9, Vol. iv, 1888.

HERBERT OSBORN,
Secretary of the Club for the Cleveland meeting.

ABSTRACT OF THE PROCEEDINGS OF THE CLEVELAND MEETING OF THE BOTANICAL CLUB OF THE A.A.A.S.

Wednesday, August 15. Meeting called to order at 9 A. M. The President, Mr. DAVID F. DAY in the chair. Rev. W. M. BEAUCHAMP was elected Secretary *pro tem.* in the absence of Prof. V. M. SPAULDING. The President opened the meeting by an address including a memoir of Dr. Gray and a recommendation that the Club be incorporated as a section of the Association.

Before the reading of the papers in the day's programme two motions were made and carried: one to the effect that the Secretary should be provided with a book for permanent records of the Club; the other, that a committee be appointed to consider the proposition in regard to uniting the Club with the biological section of the Association. As members of this committee the Chairman appointed Messrs. W. H. Hale, Thos. Meehan and J. F. Cowell.

It was also resolved that a committee should be appointed to provide for the publications of the proceedings of the Club. The committee appointed consists of Messrs. W. H. Seaman, W. H. Hale and Thos. Meehan.

Mr. Thos. Meehan read papers on "Diœcious Labiatæ," and "The Elastic Filaments of the Stamens of Compositæ;" and Mr. J. F. Cowell followed with "Observations on *Azalea nudiflora* and *Corallorhiza*."

Thursday, August 16. After the meeting had been called to order, the question of making the Club a special section of the Association, or else a sub-section of Section F, was discussed. The report of the committee was read, and in this they strongly recommended that the independent organization of the Club should be maintained. After further discussion, their recommendation was unanimously adopted.

The President then announced that an invitation had been extended to the Club, by Mr. J. D. Rockafeller, to visit his grounds. As, however,

the time of the Club was already fully provided for, it was decided that the Club as a body could not set a date when it would be possible to accept the invitation, and on motion of Prof. C. R. Barnes, the regrets of the Club at their inability to accept the invitation were ordered to be transmitted to Mr. Rockafeller.

Mr. Thos. Meehan spoke of the death of Dr. Gray and suggested that resolutions ought to be adopted by the Club in reference to it, and the following gentlemen were appointed as a committee to draft such resolutions: Messrs. C. R. Barnes, Thos. Meehan and D. S. Kellicott.

Rev. W. M. Beauchamp read a paper on "Onondaga Indian Plant Names."[1] Mr. Beauchamp also exhibited specimens of *Erythræa Centaurium*, gathered near Oswego, a plant new to most of those present.

Mr. Thos. Meehan read a paper on "Irregular Tendencies in the Tubulifloral Compositæ."[1]

Prof. E. L. Sturtevant read a paper on "Observations on the genus *Capsicum*," accompanied by numerous beautifully executed colored drawings, showing the great variations in the fruit of different varieties. The author pointed out the difficulty of distinguishing species in plants that had been long cultivated, and expressed his doubts as to the validity of certain of the genus under consideration. Mr. Meehan expressed his interest in the paper, and said he was inclined to believe that all the cultivated Capsicums were merely varieties of a single species.

The last paper was by Prof. B. E. Fernow, on the subject "What is a Tree?" In the paper the author pointed out the desirability of a generally accepted definition of the word "tree," and showed how authorities differ in regard to it. The following definition was suggested: "Trees are woody plants, the seeds of which have the inherent capacity of forming a definite trunk supporting a crown of branches."

After a discussion of the paper, the meeting adjourned.

Friday, August 17. After some preliminary business, the following resolutions, in memory of Professor Gray, prepared by the Committee appointed for that purpose, were unanimously adopted:—

Resolved, That the Botanical Club of the American Association sincerely regrets that, meeting but once a year, it should be among the last to place on record the sense of the great loss which the whole range of science suffers by the death of Professor GRAY.

Resolved, That though among the last to contribute to the wreath of sorrow with which science is everywhere crowning the memory of Dr. GRAY, this body takes a mournful pride in remembering that he was one of its honored members, and that it was as a botanist he won such eminent renown. We feel that we have a right to be among the chief mourners at his departure from the field of labor he loved so well, and in a special degree to unite our sympathies with the many thousands who miss him everywhere.

Resolved, That copies of these resolutions be forwarded to the family of

[1] Printed in Bulletin Torrey Botanical Club, Oct., 1888.

our deceased friend and to the botanical and other scientific periodicals for publication.

Papers were read by Prof. C. R. Barnes on "The Cause of the Acridity in the Corm of Arisæma," and by Mr. A. A. Crozier on "Secondary Effects of Pollination." Professor Barnes stated that it was probable that the intensely burning taste of the juice of Arisæma was due, as suggested by Stahl for the European *Arum maculatum*, to mechanical causes, *i. e.*, the irritation produced by the numerous rhaphides with which the juice is filled. Professor Barnes found that when these were removed by filtering the acrid taste was completely lost.

Mr. Crozier's paper was read by Professor Cowell, the author being absent. From the author's experiments, mostly in different varieties of apples, he concluded that the influence of foreign pollen did not extend beyond the seeds.

Mrs. H. L. Walcott exhibited the leaves of a form of choke-cherry which she described as having amber-colored berries and much shorter racemes than the ordinary form.[1]

Prof. W. R. Lazenby brought up the question as to the distinctness of the two forms of Virginia Creeper, which was discussed at some length by several members.

A letter from Dr. Geo. Vasey was read on "American Desert Plants," after which the meeting adjourned.

In the afternoon the Club made an excursion to Brighton, a suburb of Cleveland, but the flora of the vicinity did not present many novelties. One of the most interesting plants found was *Jeffersonia diphylla*, of which fine specimens were obtained in fruit.

Saturday, August 18. The entire day was devoted to a trip on the steamer "City of Cleveland," to the Put-in-Bay Islands. The trip was such a long one as to allow but little time for botanizing.

Monday, August 20. The following papers were read: by Prof. Jos. F. James on "*Dentaria laciniata* and *D. multifida*;" by Mr. F. L. Scribner on "Observations on Nomenclature" and "*Sphærella Fragariæ*;" and by Mr. Thos. Meehan, on "Peduncular Bracts in *Tilia*."

Professor James also exhibited a form of *Asclepias tuberosa* with flexuous stem and sub-opposite leaves, which he thought was sufficiently distinct to be regarded as a variety.

The committee on nominations of officers for the ensuing year reported in favor of Prof. T. J. BURRILL, of Champaign, Ill., for President, and DOUGLAS H. CAMPBELL, of Detroit, Mich., for Secretary, and also recommended that the office of Vice-President be created, and named Prof. BYRON D. HALSTED of Ames, Iowa, for the office. The report of the Committee was accepted, and the officers as named were elected.

Tuesday, August 21. Prof. W. R. Lazenby read a paper on "The Flowering Plants of Ohio," and was followed by some remarks by Mr. David F. Day on those of the vicinity of Buffalo, and by Mr. Beauchamp on the Cayuga flora.

[1] Printed in Bulletin Torrey Botanical Club, Oct., 1888.

Mr. F. Scribner read a paper upon and discussed the genus *Andropogon.*

Prof. V. M. Spaulding contributed a paper on "Changes Produced in the Host Plant by *Puccinia graminis.*" The author being absent, the paper was read by Mr. D. H. Campbell.

Prof. M. B. Waite contributed a paper on "Changes in the Local Fungus Flora of Champaign, Ill." In the absence of the author, the paper was read by Mr. Scribner.

Prof. W. J. Beal read a paper entitled "Notes on some Flowering Plants of Michigan."

At the conclusion of the meeting the Club adjourned to meet next year in Toronto, Canada.

Besides the papers read before the Botanical Club, the following botanical papers were read in Section F of the Association: "A Plea for Uniformity in Biological Nomenclature," N. L Britton; "A Study of *Hydrangea* as to the Objects of Cross-Fertilization," Thos. Meehan; "A Phase of Evolution," E. L. Sturtevant; "Notes on the Inflorescence of *Callitriche,*" Jos. Schrenk; "Hygroscopic Movements in the Cone-Scales of Abietineæ," A. N. Prentiss; "Some New Facts in the Life-History of Yucca and the Yucca Moth," Thos. Meehan; "On the Cause and Significance of Dichogamy in Flowers," Thos. Meehan; "Adaptation in the Honeysuckle and Insect Visitors," Thos. Meehan; "Comparison of the Flora of Eastern and Western Michigan in the latitude of 44° 40′," W. J. Beal; "Observations on the Succession of Forests in Northern Michigan," W. J. Beal; "The Systematic Position of the Rhizocarpeæ,"[1] Douglas H. Campbell; "Pollen Germination and Pollen Measurements," Byron D. Halsted.

The following botanical papers were read before the Society for the Promotion of Agricultural Science: "Peculiarities of the Plants of Northern Michigan," W. J. Beal; "Notes on Flowering Plants of Ohio," W. R. Lazenby; "Potato Flowers and Fruit," Byron D. Halsted; "Tomato Flowers and Fruit," Byron D. Halsted; "A Further Study of the Dandelion," E. L. Sturtevant; "Successful Treatment of Black Rot," F. L. Scribner.

DOUGLAS H. CAMPBELL.

[1] Printed in Bulletin Torrey Botanical Club, Oct., 1888.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

THE THIRTY-SEVENTH MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE WAS HELD IN THE CENTRAL HIGH SCHOOL BUILDING, CLEVELAND, OHIO.

AT ten o'clock on Wednesday morning, August 15, the Association met in GENERAL SESSION in the large hall of the High School, and was called to order by President S. P. LANGLEY of Washington who invited the Rev. Dr. C S. BATES of Cleveland to open the session with a prayer, which was offered as follows:—

Our Father, who art in heaven, Hallowed be thy Name. Thy kingdom come. Thy will be done on earth, As it is in heaven. Give us this day our daily bread. And forgive us our trespasses, As we forgive those who trespass against us. And lead us not into temptation; But deliver us from evil: For thine is the kingdom, and the power, and the glory, for ever and ever. Amen.

We thank Thee, our Heavenly Father, for every revelation of truth which Thou hast made to the children of men. We pray that Thy blessing may rest upon every one who is seeking to know more clearly the meaning of any word of any of those revelations. But especially do we pray that Thou wilt bless the members of this Association and the work in which they are engaged. May the fruit of their labor be full of ministration to the needs of men; giving to their bodies fuller life and larger power, and to their souls a clearer vision of the greatness of Thy wisdom, and the glory of Thy love.

Direct us, O Lord, in all our doings, with Thy most gracious favour, and further us with Thy continual help; that in all our works begun, continued, and ended in Thee, we may glorify Thy holy Name, and finally, by Thy mercy, obtain everlasting life; through Jesus Christ our Lord. Amen.

The grace of our Lord Jesus Christ, and the love of God, and the fellowship of the Holy Ghost, be with us all evermore. Amen.

PRESIDENT LANGLEY, with a few graceful remarks, resigned the chair to the President elect, Major J. W. POWELL of Washington, who on accepting it said:—

Over this society you have called me to preside. For the honor you have conferred upon me I am most profoundly grateful. When I remember the great men who have presided over this body I am deeply sensible of the magnitude of the trust imposed upon me, and would gladly be relieved from its duties and would surrender its honors to purchase immu-

nity from the ordeal through which I must pass, for I feel that I am very inadequately prepared to fill the position. It is with this feeling that I invoke your charitable assistance to secure for the deliberations of this meeting that wisdom and harmony which will make the society for another year an effective agency for the advancement of science.

DR. CADY STALEY, president of the Case School of Applied Science, Cleveland, was introduced and delivered the address of welcome on behalf of the LOCAL COMMITTEE as follows:—

It is thirty-five years since our citizens have had the privilege of welcoming to Cleveland the American Association for the Advancement of Science. During those years the advancement of science has been marvellous. The discoveries and inventions within that time have exceeded the dreams of the wildest visionary who met with the society at its last meeting in this city. For this rapid advance of science, in both theory and practice, the world is greatly indebted to the members of this Association. If there are any here of that company who met with the Association in '53 they will testify that the growth and development of the city have not been less marvellous than has the advance of science in the same period. Then there were in the city about 26,000 inhabitants. To-day there are about ten times that number, and to a large extent the city owes its magical growth and wonderful prosperity to the advance of science, and its application in the arts and in manufactures. It would be difficult to find a city in which a larger proportion of the inhabitants are interested, directly or indirectly, in pursuits which depend upon scientific methods and processes.

It is peculiarly fitting that this association of scientists should meet in Cleveland—a city which owes so much to the practical application of science in various fields.

No guests can be more welcome than those whose time and talents are employed in forwarding interests of such paramount importance to this city.

To our own citizens this meeting is an event of no slight importance. The value of such meetings in any community, in creating an interest in science and in scientific research, is not easily overestimated.

On the other hand, we trust that to the members of the Association, their coming to this city may not be altogether without profit. While the work of deducing scientific laws is of itself of intense interest, and purely theoretical results are worth all that they cost in time and labor, still the practical application of scientific laws and theories has, for a large majority of even scientific men, the greater charm. I doubt if there is another city in this broad land where applied science in its various fields can be so advantageously studied.

To simply name the establishments which depend upon scientific methods and processes for their administration would weary your patience. I have often thought of what an opportunity there is here for some one with the gift of story-telling to write a modern version of the "Arabian Nights" in which shall be told more marvellous things than Scheherazade recited for

the amusement of the bloodthirsty Sultan, and yet all strictly true. The most wonderful story in that book full of wonders is that of "Aladdin and his Lamp." According to the story, Aladdin had but to rub the lamp to summon the genie, who—to use the phrase of the old story teller— was "ready to obey him as his slave, the slave of all who hold the lamp in their hands." In short, the genie would do whatever Aladdin told him to. Aladdin had only to draw on his imagination.

And now having cut loose from all pretence of adhering to plain, unromantic facts, the story teller gives rein to his imagination, to see to what lengths his untrammelled fancy can carry him. In his wildest flights the genie of the lamp builds for Aladdin a splendid palace and furnishes it gorgeously; provides him with magnificent apparel and sumptuous banquets; brings him vessels of gold and silver, and jewelry of rare workmanship, set with costly gems. Now the point in which the modern version surpasses the ancient one is that all this has actually come true in Cleveland.

You can see on our streets splendid palaces produced and furnished by the slave of the lamp; and this same slave has brought enormous wealth, not to one only but to several modern Aladdins. Aladdin could summon the genie only by rubbing one particular lamp, and if some one else secured that lamp Aladdin could no longer control the genie. But in these latter days we can summon the slave by rubbing other things besides a lamp, and we have found out many ways of commanding the presence and service of the wonder-working genie. One of our Cleveland Aladdins employs a windmill for that purpose. Aladdin stood much in awe of the slave of the lamp, and called for his services only upon rare and important occasions. But we have grown more familiar with him, and have made him generally useful. He draws wagons, drives machinery, melts refractory substances, combines and separates chemical elements, produces light and heat at our pleasure, runs errands, carries messages,—in fact we could hardly keep house without him.

I can imagine a traveller from Cleveland walking the streets of a Persian city and falling in with one of the professional story-tellers, who still amuse the people of the East by spinning marvellous yarns of the "Arabian Nights" variety. The traveller listens to one of the Persian's stories and then proposes to tell one himself. He begins by stating that what he will tell will be a plain unvarnished tale of what could be seen any day in his native town, and not the wild flights of Eastern fancy. Then he tells of the manipulation of iron at the rolling mills, the steel works and the forges; of the marvels of mechanism turned out by Warner & Swazey; of the ingenious contrivances employed in the various factories; of the wonders wrought by chemical processes; of the work of the modern slave of the lamp—the electric lights and motors, the telephone and the phonograph. And, as the traveller proceeds, the expression on the old Persian's face changes from one of lively interest and astonishment to that of dejection and defeat. He feels himself completely outdone in narrating marvels. Then from the folds of his robe he draws and extends to the traveller an

lluminated card upon which is inscribed in beautiful Persian characters—"I am something of a liar myself."

I need carry the idea no further. You can each write your own version of the "Modern Arabian Nights," and keeping strictly within the realm of accomplished facts as exemplified in this city, you may tell of more wonderful things than ever Eastern story-teller conceived in the wildest flights of his imagination. We hear about the triumphs of science until the subject seems trite, and yet no one man can fully realize all the wonderful things which have been accomplished in the many and varied fields of scientific investigation; the rapid advances of the present; or the possibilities of future achievement.

I look forward to this meeting of your Association for new revelations in the fields of scientific research; for help in once more getting abreast of the thought and work of the day; and for renewed inspiration in keeping step with the march of progress, and in doing our part in the rapid advance of science. The citizens of Cleveland are interested in the work of this Association; they are ready to do what they can to make this a successful meeting; and in their behalf I extend to you a hearty welcome.

It was expected that Mayor Babcock would deliver the address of welcome on behalf of the city, but in his absence the duty was admirably performed by the local secretary, Dr. ELROY M. AVERY. In the course of his remarks, he said: You have been charmed by this charming story of our city, but it is incomplete, for what is Hamlet with Hamlet left out? I am not the mayor of the city, but I am his humble and obedient slave. I give you welcome to this beautiful city in the name of its quarter of a million of inhabitants. In the name of the municipality, I bid you each and every one a cordial welcome to the Forest City.

President POWELL replied as follows:

In behalf of the American Association for the Advancement of Science, I give you most hearty thanks for the welcome you have extended to the society, and for the generous provision that the citizens of Cleveland have made for its meeting. It gives us great joy to meet in this beautiful city. Cleveland stands on a terrace fashioned by the ice of a continental glacier and the waves of an ancient lake, and is surrounded on one side by beautiful Devonian hills and on the other by crystal waters. We remember that here at Cleveland the waves of Erie beat in solemn rhythm against the grave of Garfield, the statesman and scholar who, in all his public life, sought earnestly and successfully the advancement of science. Our meeting here is a pilgrimage to his tomb.

In the early years, after the foundation of the government, a few great men were interested in the philosophy of science, in its facts and in research. Franklin was a physicist, Jefferson a naturalist, and Gallatin an anthropologist. These, and other great men of the time, drank deeply at the fountain of science; but they were statesmen, or they had other callings, and made the advancement of science a secondary purpose. But

slowly scientific men rose, one after another, who devoted their entire energies to research. In the last generation, a galaxy of great scientific men appeared in the American firmament: Henry, Bache, Pierce, the Rogers brothers, Gray, Baird, and many others. These men devoted their lives to research and instruction in science. In 1840, they organized the Association of Geologists and Naturalists, and in 1848 they transformed that society into the American Association for the Advancement of Science. Since that time the society has embraced in its membership all, or nearly all, of the scientific men of America, and in the list of its officers—presidents, vice-presidents and secretaries—the names of many of the illustrious scientific men of the country are enrolled.

Most of the great men of that generation have sailed away on the unknown sea. A few only, like Dana, Hall, Newberry and Lesley, remain to guide in our councils and to cheer on the labors of the present generation of investigators. The society which they organized has grown with the growth of the country and the far more rapid growth of science, until it embraces a membership that constitutes a vast corps of laborers who occupy the border-land of knowledge which is the field of research. To enumerate in systematic order the fields of research occupied by the various members of this association would be to formulate a classification of the sciences.

Atoms, mountains and worlds, with all inorganic bodies and all inorganic motions, are to be examined in the study of the inorganic realm.

The phenomena of nature are qualitative and quantitative, and out of quantitative relation the abstract science of mathematics has been developed, and this science of measurement is rapidly being applied to the qualitative sciences. There are many men engaged in mathematical researches.

There are members who study the stars, compute their distances and determine their courses, and who are seeking to solve the mysteries of the constitution of the bright sun and the pale moon; for on the chariot of light they drive through the storms of the greater orb and explore the dim fields of the lesser.

Others with patient labor seek to discover the nature of light, of electricity and of gravity — the mysterious force that impels the universe.

There are others, many others, who are investigating the minute constitution of matter and determining its many forms. These forms are forever changing. The crystals of the rocks that make up the mountain mass are dissolved and their atoms redistributed in new forms, and chemical changes are in progress wherever human investigation can penetrate. The tree grows and decays, and man is but a form—a mold, through which streams of atoms pour in waves of chemic change. So the chemist studies the laws which govern the constitution of bodies and under which they are forever in flux.

There are others who are studying the molar motions and mechanical powers by which waters are made to turn mills, winds to waft vessels and steam to drag cars.

There are others who study the atmosphere which bathes the earth.

They study the coming and going of storms, where fierce cyclones are born, how the cold wave creeps from polar regions and the hot wave from the tropics.

There are others who are studying the surface of the earth—the lands and seas in all their places and forms. At the far North there is a region walled by ice a million and a half square miles in extent, but even into this land of ice they penetrate. About the South pole there is an area of seven million square miles inclosed by a barrier of ice — an unknown region into which the modern scholar is bound to enter. Between these walls the whole habitable earth is spread; and they are exploring all its seas, navigating all its rivers, climbing all its mountains and threading all its cañons.

A great army of men is engaged in the study of the constitution of the earth—the origin of mountains and valleys, of hills and prairies, of volcanoes and geysers, of cataracts and caves and of rivers and lakes and seas. They examine the great coral reefs and islands of the sea, and they study the great coral rocks of the land — the fossil reefs and islands of ages gone by. Climbing among cliffs they study the anatomy of dead volcanoes, and climbing to the brink of craters they study the physiology of living volcanoes. If an earthquake rends the rocks, they measure the vibrations of its waves, and with the eye of science penetrate to the centre of the disturbance and draw upon their maps the lines of weakness in the crust of the earth. They follow the sands that are washed by storms from the mountain sides until they find them built by the sea into islands and coasts. With microscope and crucible they study the constitution of granite, basalt, trachyte and other rocks wherein appear the crystal forms of many minerals. They show that the grand mountain form, with its crags and peaks and grottoes, where forests stand, where lakes are embosomed and where cataracts flash in the sunshine, is indeed an aggregation of many gems beautiful in form and brilliant in color.

But man is not satisfied with the knowledge which comes with the study of the inorganic realm; he essays to solve the mysteries of life.

An army of men is engaged in the investigation of vegetal life. They find minute but beautiful plants that grow as dust on polar ice; in dank fields they find fungi, on rocks they find mosses, on the waves they find sea-weeds, on tropical trees they find orchids, on the prairies they find asters, in the savannas they find lilies, in the jungle they find palms, in the forests they find oaks, on the mountain flanks they find sequoias; and they study all these forms and a thousand more, and out of their study grows the science of botany. Then they must know how these forms became, and they trace their origin in the dim past, and they exhume the forms of plant life from the tombs of ancient meadows and groves.

Then another army of men is engaged in the investigation of animal life, and they find the land and the sea teeming with varied forms. In the sea the coral animals grow and build their weird structures. On the bottom and shores of the sea mollusks crawl, carrying with them their pearl-lined homes. There are mollusks in all the lakes, in all the rivers, in all the brooks and in all the ponds, and they wander over the lands and climb the trees. On the lands there are crawling worms and in the seas crawl-

ing articulates, and the world is covered with crawling insects. The ants live in cities of their own building, the bees live on the clover blossoms and the butterflies play among the roses. The fishes swim in the waters, the reptiles crawl in the marshes, the birds fly in the air, and the beasts roam over the land. These animal forms are studied and classified, and we have systematic zoology. But this is not all of zoology. In the life of every living thing there is a wonderful history of transformation; so zoologists study the birth, growth and death of animals. Then they discover the origin of present tribes, by investigating the forms of life that have existed in the past, and they call upon the rocks to reveal their evolution.

Man essays to learn the marvellous structure of the human form, and the working of this complicated organism, and the processes by which the materials of the world are transformed into brawn and brain, and by which the powers of the dead universe are transformed into life. This study gives us the science of human biology. Having learned how men live, scholars seek to learn how men may live longer. In his course of conquest, man has transformed tribes of wild beasts into herds and flocks; he makes the cataract his slave and laughs at the lightning; a multitude of enemies by which he was once surrounded have now become his friends; in his puissance he seems to have conquered all; but while he has subdued many of his great enemies, he is surrounded by hosts of infinitesimal foes. He fears no attack of the lion, but he surrenders in death at the attack of the microbe, yet by light of science he seeks to disarm and destroy these infinitesimal foes.

Stars in their orbits, atoms in their affinities, plants in their growth, animals in their functions, present fields of research so vast, problems so recondite, that it might be supposed that the human reason would tremble under the load; but in fact the burthen thus carried has trained the powers of the mind to greater feats of reason, and man, triumphant in his researches through the universe of matter and life, essays at last to study himself, and he is organizing the science of anthropology. A galaxy of stars, a mountain of crystalline forms, a forest of plants, an island of life, are indeed subjects worthy of contemplation, but man, living in many lands, skilled in many arts, organized into many tribes and nations, speaking many languages, thinking many thoughts, propounding many philosophies, is the grander theme of anthropology.

At the threshold of anthropology we meet with the science which seeks to explain how life is transformed into mind; how the blush on the apple is transformed into the blush on the cheek, how the flight of the bird is transformed into the flight of the imagination, and how the brain with the nervous system is made the organ of this mind. How wonderful it is that sensation, perception, reasoning, memory, feeling, emotion and will appear as functions of this mysterious organ. In these researches many men are employed.

Man has sought out many inventions; thus we have the arts. To eat the berry from the bush, to munch the nut from the tree, to devour the clam on the bank, or to waylay a terrified hare, is to do no better than

the brute; but to plant an orchard, to cultivate and stock a fishery, or to shepherd flocks, is to play the better part of man. To hide in caves is to compete with wolves; to construct homes is to become a creator. To seek protection from the pelting storm under the foliage is but a bird's device, but to clothe himself in raiment is the invention of man. So the arts of life are developed. First, man learned to use the mechanical powers; then he learned to utilize the subtle energies that pervade the universe. So all of the industrial arts have been invented. First he learned dancing, then rhythm, then melody, then harmony; until at last the fireside is cheered by song and the multitude filled with supreme emotion by choir and orchestra. At first he painted pictures on the skins of beasts and etched them on the rocks, and at last he paints pictures that mirror life and landscapes that mirror nature. At first he scratched signs on the bark of trees to mark the simple events of his journey; having invented letters and printing, he fills libraries with the records of his knowledge. So arts industrial and arts æsthetic were evolved, and the history of all these inventions has become the theme of modern investigation; and men excavate caves, open ancient graves, examine mounds and sift the debris of ruined cities for the materials of archæology.

There are many languages, and there are many men who study these languages, and they have created the science of philology. In the beginning man might shout his words across the creek, but to speak to his friend across the river he must swim the river; but now he speaks across the sea. There was a time when a spoken word died on the breeze, but now it is recorded for all time.

The evolution of animal life is by the method characterized as the survival of the fittest in the struggle for existence. It is a brutal method of progress. It is progress by pain and death. It means to starve the weak, to torture the unfortunate, and to rejoice in all calamity, for so progress is made in the animal world. The rule of nature, under the law of the survival of the fittest, involves the supremacy of claw and beak and poisoned fang. But man invents a new method of progress by human endeavor. He has organized institutions to establish peace, to secure justice and to promote mutual assistance. The organization of society and the establishment of laws to secure justice effect a repeal of the biotic law,—" the survival of the fittest in the struggle for existence." The rise and fall of states, the growth of forms of government, the development of law, and the interdependence of industries are subjects of inquiry with many men; and so researches relating to human institutions are organized, and a multitude of men are engaged in this work.

Man is an animal in body, stomach, and legs; but then he is an animal with opinions, and forever he has been systemizing these opinions into philosophies. In the earliest philosophy everything was endowed with life and deified—stones, trees, fountains, forests, beasts, winds, waves, and stars; and the mysteries of the universe were explained by making all these things intelligent actors. From this, the earliest philosophy of the lowest savage, it is a long way to the philosophy of science, and there have been many stages. That hollow dome, the firmament, has become infinite space;

the wind, that was at first believed to be the breath of beasts stationed at the four corners of the earth, has at last become the circumambient air in motion under physical laws; the flat plain of earth has become a globe; astrology has become astronomy; alchemy has become chemistry; witchcraft has become medicine; beast gods have become domestic animals; and nature gods have become energies that can be used as the servants of man. The history of these opinions and of the philosophies into which they are woven is now a theme for the investigation of many men.

So the members of this society are prosecuting investigations in the realms of motion, life, and mind; and there is such a division of labor that every great science included in these realms has its devotees. It is a goodly work. It is a grand work.

Dr. JULIUS POHLMAN, of Buffalo, having been nominated by the COUNCIL, to fill the vacancy caused by the resignation of the General Secretary elected at the New York Meeting, was elected General Secretary for the Cleveland meeting.

The GENERAL SECRETARY announced that one hundred and twenty-six new members had been elected by the COUNCIL since the close of the New York Meeting, and that one hundred and thirty-seven papers had been presented for the present meeting.

On recommendation of the COUNCIL, it was voted that the forenoon sessions be held from 10 to 12½ o'clock, and that the afternoon sessions begin at 2 o'clock, standard time.

The PERMANENT SECRETARY stated that since the last meeting the Association had lost two of its original members: Prof. SPENCER F. BAIRD, who died August 19, 1887, was the first Permanent Secretary of the Association and held the office for the 5th, 6th and 7th meetings, the last being the meeting of 1853, held here in Cleveland.

Prof. ASA GRAY, the other original member, was President of the Indianapolis meeting in 1871. He died January 30, 1888.

Notices had also been received of the death of the following members.

HARDWAY H. DINWIDDIE, College Station, Tex. (32). Died Dec. 11, 1887.

GEORGE NEWELL DOGGETT, Chicago, Ill. (33). Died Jan. 15, 1887.

WALTER ANGUS DUN, Cincinnati, Ohio (31). Died Nov. 7, 1887.

EZEKIEL B. ELLIOTT, Washington, D. C. (10). The first Vice President of Section I. Died May 24, 1888.

JOSEPH FICKLIN, Columbia, Mo. (20). Died Sept. 6, 1887.

A. E. HEIGHWAY, Cincinnati, Ohio (29). Died Jan. 24, 1888.

HENRY A. HOMES, Albany, N. Y. (11). Died Nov. 3, 1887.

ROLAND DUER IRVING, Madison, Wis. (26). Died May, 1888.

ETHAN PENDLETON LARKIN, Alfred Center, N. Y. (33). Died Aug. 23, 1887.

HENRY CARVILL LEWIS, Philadelphia, Pa. (26) Died July 21, 1888.

MOSES LYFORD, Springfield, Mass. (22). Died Aug. 4, 1887.

A. R. McCUTCHEN, Atlanta, Ga. (25). Died Nov. 21, 1887.

DONALD MACGREGOR, Houston, Tex. (33). Died in October, 1887.

ELISHA N. SILL, Cuyahoga Falls, Ohio (6). Died April 26, 1888.

SILAS STEARNS, Pensacola, Fla. (28). Died Aug. 2, 1888.

JAMES STEVENSON, Washington, D. C. (29). Died July, 1888.

LEANDER STONE, Chicago, Ill. (32). Died April 2, 1888.

ALGERNON SIDNEY SULLIVAN, New York, N. Y. (36). Died Dec. 4, 1887.

ROBERT N. TAYLOR, Tollesboro, Lewis Co., Ky. (37). Died Aug. 13, 1888.

H. F. WALLING, Cambridge, Mass. (16). Died April 8, 1888.

SAMUEL D. WARREN, Boston, Mass. (29). Died May 11, 1888.

P. O. WILLIAMS, Watertown, N. Y. (24).

AMOS H. WORTHEN, Springfield, Ill. (5). Died May 6, 1888.

By direction of the COUNCIL the PERMANENT SECRETARY gave a statement of the financial condition of the Association. After announcements by the LOCAL SECRETARY, the GENERAL SESSION adjourned to meet in Sections.

In the evening, a GENERAL SESSION was held in the hall of the High School, at which a large number of citizens were present, and the Retiring President, Professor LANGLEY, delivered his address, entitled "The history of a scientific doctrine."

The GENERAL SESSION on Thursday was called to order by the PRESIDENT at 10 o'clock A. M.

Prof. C. J. H. WOODBURY of Boston, was elected Vice President of Section D to fill the vacancy caused by the absence of Prof. CALVIN M. WOODWARD. Notice was given that the WESTERN RESERVE HISTORICAL SOCIETY had invited the Association to visit its rooms between 1 and 6 o'clock P. M. on week days and from 3 to 6 o'clock on Sunday afternoon;— and that the CLEVELAND AUTOMATIC REFRIGERATOR Co. invited the Association to an inspection of its works on Friday, from 5 to 7 o'clock P. M.

At the GENERAL SESSION on Friday an invitation from Dr. CLARKE of Berea was received, for the inspection of his unique collection of fossil fishes. Several announcements were made regarding the Saturday's excursion.

In the evening Major POWELL delivered a lecture on "Competition as a factor in Human Progress."

At the GENERAL SESSION on Monday morning, Professor STALEY of the Case School of Applied Sciences, told why the Association had not been invited to visit the school, the cause being that only the bare walls of the new building were ready for inspection.

In the evening, Prof. T. C. MENDENHALL lectured on "Japanese magic mirrors."

The GENERAL SESSION was called to order on Tuesday morning by the Senior Vice President, Prof. GEORGE H. COOK. The names of the officers for the ensuing year, selected by the Nominating Committee, were presented as follows:

PRESIDENT.

T. C. MENDENHALL of Terre Haute, Ind.

VICE PRESIDENTS.

A. **Mathematics and Astronomy**—R. S. WOODWARD of Washington.
B. **Physics**—H. S. CARHART of Ann Arbor, Mich.
C. **Chemistry**—WILLIAM L. DUDLEY of Nashville, Tenn.
D. **Mechanical Science and Engineering**—ARTHUR BEARDSLEY of Swarthmore, Pa.
E. **Geology and Geography**—CHARLES A. WHITE of Washington.
F. **Biology**—GEORGE L. GOODALE of Cambridge, Mass.
H. **Anthropology**—GARRICK MALLERY of Washington.
I. **Economic Science and Statistics**—CHARLES S. HILL of Washington.

PERMANENT SECRETARY.

F. W. PUTNAM of Cambridge, Mass. (office Salem, Mass.). Holds over.

GENERAL SECRETARY.

C. LEO MEES of Terre Haute, Ind.

SECRETARY OF THE COUNCIL.

FRANK BAKER of Washington.

SECRETARIES OF THE SECTIONS.

A. **Mathematics and Astronomy**—G. C. COMSTOCK of Madison, Wis.
B. **Physics**—E. L. NICHOLS of Ithaca, N. Y.
C. **Chemistry**—EDWARD HART of Easton, Pa.
D. **Mechanical Science and Engineering**—JAMES E. DENTON of Hoboken, N. J.
E. **Geology and Geography**—JOHN C. BRANNER of Little Rock, Ark.
F. **Biology**—AMOS W. BUTLER of Brookville, Ind.
H. **Anthropology**—W. M. BEAUCHAMP of Baldwinville, N. J.
I. **Economic Science and Statistics**—J. R. DODGE of Washington, D. C.

TREASURER.

WILLIAM LILLY of Mauch Chunk, Pa.

AUDITORS.

HENRY WHEATLAND of Salem, Mass., THOMAS MEEHAN of Germantown, Pa.

On motion, it was voted that the SECRETARY be directed to cast a ballot for the election of the nominees, which was done and they were declared elected as officers for the next meeting.

On the recommendation of the Nominating Committee, it was voted to accept the invitation for Toronto and hold the next meeting there, beginning with the meeting of the COUNCIL on Tuesday, Aug. 27, 1889, and the first GENERAL SESSION on Wednesday, Aug. 28.

On recommendation of Section C, Dr. F. A. GENTH of Philadelphia, Pa., was elected an Honorary Fellow of the Association.

The SECRETARY announced that the following had been elected Fellows by the COUNCIL:—

Baylor, James B., U. S. Coast Survey, Washington, D. C. (33). **A**
Bigelow, F. H., Racine, Wis. (36). **A**
Boas, Franz, New York (36). **H**
Bransford, John Francis, Washington, D. C. (36). **H**
Campbell, Douglas H., Detroit, Mich. (34). **F**
Collingwood, Francis, Elizabeth, N. J. (36). **D**
Cushing, Henry Platt, Minn. Normal School, Mankato, Minn. (33). **E**
Denton, James E., Stevens Institute, Hoboken, N. J. (36). **D**
Green, Arthur L., Lafayette, Ind. (33). **C**
Howe, James Lewis, Louisville, Ky. (36). **C**
Johnson, Arnold Burges, Washington (35). **B**
Kemp, James F., Ithaca, N. Y. (36). **E**
Keiser, Edward H., Bryn Mawr, Pa. (35). **C**
Leavenworth, F. P., Haverford College, Pa. (30). **A**
Matthews, Washington, Washington, D. C. (37). **H**
McGill, John T., Vanderbilt University, Nashville, Tenn. (35). **C**
Menocal, A. G., Washington, D. C. (36). **D**
Nason, Frank L., Geological Survey of New Jersey, New Brunswick, N.J. (36). **E**
Nicholson, H. H., University of Nebraska, Lincoln, Neb. (36). **C**
Seymour, Wm. P., Troy, N. Y. (19). **H**
Sharpless, Isaac, President Haverford College, Pa. (33). **A**
Simonds, Frederick W., Fayetteville, Ark. (25). **E F**
Snyder, Henry, Oxford, Ohio (30). **B C**
Staley, Cady, Case School of Applied Science, Cleveland (37). **D**
Stevenson, J. J., University of City of New York (36). **E**
Thompson, Elihu, Lynn, Mass. (37). **B**
Tittman, O. H., Washington, D. C. (24). **A**
Towne, Henry R., Stamford, Conn. (33). **D E**
Tucker, Willis G., Albany, N. Y. (29). **C**
Ware, William R., Columbia College, New York (36). **D**
Warner, Worcester R., Cleveland, O. (33). **D**
Weber, Henry A., Ohio State University, Columbus, O. (35). **C**
Willson, R. W., Cambridge, Mass. (30). **A**
Wilson, Thomas, Washington, D. C. (36). **H**
Worthen, William E., New York (36). **D**

The Committee on the Registration of Births, Marriages and Deaths, was discharged.

The Committee on Stellar Magnitudes was discharged at its own request.

The Committee on Anatomical Nomenclature, finding its work difficult, asked for an extension of time to prepare a report. Granted.

The Committee on Physics Teaching made a report and was continued.

The Committee on the Reduction of the Tariff on Scientific Books and Apparatus made a report and was continued.

The Committee on the Preservation of Archæologic Monuments on the Public Lands made a report and was continued.

The Committee on International Congress of Geologists reported through Prof. G. H. COOK.

Prof. C. H. HITCHCOCK moved that the report be accepted as presented and the committee be continued; seconded by Prof. N. H. WINCHELL and carried unanimously.

It was moved by Professor HARKNESS that all the reports be published in the Proceedings of the Association. Voted.

The Secretary announced that the COUNCIL recommended the formation of the following committees: and the same were duly elected.

On Chemistry Teaching. W. H. Seaman, William L. Dudley, H. W. Wiley, W. O. Atwater and W. A. Noyes.

On Water Analysis. G. C. Caldwell, J. W. Langley, J. A. Myers, W. P. Mason, R. B. Warder and W. H. Seaman.

On Organization of a National Chemical Organization. A. B. Prescott, Alfred Springer and Edward Hart.

Dr. A. B. Prescott was appointed substitute for Mr. Scudder on the committee on indexing chemical literature.

On a Universal Language. Horatio Hale, H. W. Henshaw, Alex. McFarland.

Mr. P. H. DUDLEY, the Honorary Special Agent of Transportation, made a report which was read and ordered to be printed in the Proceedings.

The concluding GENERAL SESSION was held on Tuesday evening August 21, in the hall of the High School. Vice President COOK presided in the enforced absence of President POWELL. The members of the Association were in good humor and were bound to show their appreciation of the hospitality and kindness of the Cleveland people which has been shown in so many ways during the meeting. Secretary POHLMAN announced that C. A. MARKS of Cleveland had been elected a member of the Association and then presented the following report from the Section on Anthropology, which was unanimously adopted:

Resolved, that we heartily commend the effort of the Western Reserve Historical Society to secure Fort Hill to the people of Ohio. That we appreciate highly the importance of preserving to all time in perfect condition one of the wonderful remains of antiquity, so fast disappearing, and

recommend to the citizens of Ohio the work already begun at the Serpent Mound in that state by citizens of Massachusetts.

This resolution brought forth remarks on the necessity of preserving the wonderful relics of prehistoric man which exist in Ohio and also in other parts of the country.

Professor PUTNAM said that it had been discovered that some earthworks in Asia bear a strong resemblance to those in Ohio and it was most important to preserve the latter, in order that comparisons be made in the future. He called particular attention to the duty of the citizens of the state, upon whom largely rested the responsibility of rescuing from destruction these monuments of another race. He called upon the people of Cleveland to furnish funds for that purpose. Fort Hill, he said, bears evidence of great antiquity, and it has a peculiar interest for all students of archæology.

Mr. THOMAS WILSON of Washington, called attention to the methods by which European nations take care of their prehistoric monuments and emphasized the necessity for similar proceedings in the United States.

The following resolutions were then read:—

Resolved, that we, the members of the American Association for the Advancement of Science, hereby desire to express our heartfelt thanks to the LOCAL COMMITTEE selected by the citizens of Cleveland to look after our well-being; that we duly appreciate the work done for our individual comfort and pleasure and also that performed for the success of the meeting by the subcommittees; that we gratefully record our obligations to the genial secretary, Dr. ELROY M. AVERY, who with untiring energy has performed the laborious and often onerous duties of his important office.

Resolved, that the thanks of this Association are due and are hereby heartily tendered to the LADIES of the Local Committee and their friends for their thoughtfulness, their devoted attention and their generous hospitality to members and their families during our stay in this beautiful city.

We offer our thanks to the Managers of the Excursion to Kelley Island and Put-in Bay, who had the rare foresight and wisdom to select for that occasion the most lovely day of the week, who provided for us pleasant and amiable company and who, without forgetting our material wants, delighted our ears with strains of sweet song and music rendered by a corps of artists who gave evidence that Cleveland is a city where the 'fittest survive' in many different forms. This favorable combination made the occasion one that will long linger in the memory of the fortunate ones who participated. We particularly desire to place on record our obligations to Mr. and Mrs. S. T. Everett, Mr. and Mrs. Stewart Chisholm, Mr. and Mrs. T. D. Crocker, Mr. and Mrs. R. K. Winslow, Mr. and Mrs. George H. Ely, Mr. and Mrs. W. S. Tyler, Mrs. J. E. Carey, and Mrs. J. F. Clarke, for the kindness shown us in opening their homes to us, giving us an op-

portunity to see some of the most beautiful residences on one of the finest avenues in our country. Likewise to Mr. W. J. Gordon for opening his beautiful and extensive park for our enjoyment, and to Mr. and Mrs. L. E. Holden for the delightful reception given us at their charming summer residence on the lake shore.

The facilities of the High School, making it possible to hold the meetings of all the sections under one roof, are duly appreciated, and that this building was placed at the disposal of the Association for our meetings calls for a cordial expression of thanks to the Board of Education.

Our thanks are due to the Daily Press of Cleveland for the care which has been taken to publish abstracts of papers and addresses and long series of editorials and notices, by which the work of the Association has been placed before the public, again demonstrating its valuable aid in the diffusion of knowledge. To the Western Reserve Historical Society which opened its rooms at hours most convenient to the members of the Association, giving thereby an opportunity to examine the valuable collections accumulated by the energy and perseverance of a few far-sighted members of the community, we also offer our appreciative thanks.

To the Western Union Telegraph Company for placing its wires at the disposal of the members of the Association for the transmission of social messages; to the various Railroad Companies for granting reduced return fares; to the Telephone Exchange for facilities granted, and to a host of citizens of Cleveland who have contributed to the comfort and pleasure of the members of the Association during this our thirty seventh meeting, we offer our most grateful acknowledgment and our heartfelt thanks.

These resolutions were warmly seconded by Messrs. MASON, MUNROE, JASTROW, PEET, MENDENHALL, MORSE and BRINTON, and carried unanimously.

Professor HERBERT C. FOOTE of Cleveland, responded in behalf of the ladies of the Local Committee, and President CADY STALEY delivered a few timely remarks, wishing the visiting scientists godspeed and stating that, as he had predicted at the opening session, the people of Cleveland had been benefited by the meeting of the Association.

The PERMANENT SECRETARY then gave his usual statistical report of the meeting.

Vice President COOK then declared the meeting adjourned, and expressed the hope that all would meet again in Toronto in August, 1889.

JULIUS POHLMAN,
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

THE thirty-seventh meeting of the Association was guided by another generation than that of 1853, when its seventh meeting was held in Cleveland. Fortunately for the Association it yet has among its most honored members several who took part in the former meeting, and it is well that the principles of its founders have been retained, so that the necessary changes, required to meet the demands made by the great advances in science, have been brought about with due caution and have not turned the Association away from the great object which led to its existence.

As in the development of the Association itself, so in the city of Cleveland, thirty-five years have made great changes in national progress, in development of resources, and in the wonderful application of science to the arts for which the city is noted. This was well told at the meeting by Dr. Staley, the President of the Case School of Applied Science; a school which is destined to foster scientific research in its widest sense, by carrying out the objects of its generous founder in properly applying science for the benefit of man. With all this great development in material progress and in wealth, it is very apparent from the educational, historical, literary, and artistic institutions of the city, that culture has not lagged behind; while the old-time generosity of the people was shown by the cordial greeting and the kindly care given to the members of the Association at the meeting of 1888. That the next generation will find still further progress, with ample endowments for research in all departments of science, can be foretold in a city where so much of its wealth has been secured through the application of scientific principles; but it is to be hoped that their foundations will not be delayed too long, and that this beautiful city by the lake will not be outstripped by her rivals in culture and progress.

Several unfortunate circumstances resulted in a smaller attendance of members at the meeting than was reasonably expected. The change of date, made necessary by a great gathering in the city which took place at the time first assigned for the meeting of the Association, and the convening of the National Educational Association at a distant point, both tended to decrease the number of members in attendance at the Association. The natural aversion to going into the interior of the country in August always tells against such meetings, but, as numbers is not the primary object of the Association, a small meeting must not be looked upon as necessarily an unsuccessful one, and certainly this was not the case at Cleveland.

The three hundred and forty-two registered as members and associates in attendance at the Cleveland meeting were from the following places:—

City of Cleveland, not including the citizens who attended the meeting without joining the Association, 82; from the rest of Ohio, 43; from New York, 56; Washington, 40; Massachusetts, 19; Pennsylvania, 18; Illinois, 16; Michigan, 16; Wisconsin, 12; Iowa, 11; Indiana, 11; New Jersey,

10; Canada, 7; Connecticut, 6; Nebraska, 5; Kansas, 5; Kentucky, 4; Virginia, 4; Mississippi, 4; Tennessee, 3; Minnesota, 3; Missouri, 3; Maryland, 3; Arkansas, 2; Texas, 2; Maine, 1; New Hampshire, 1; Rhode Island, 1; West Virginia, 1; North Carolina, 1; California, 1; Paris, 1.

Of the 182 members elected since the New York meeting, 130 have perfected their membership, also two who were elected at New York; and twenty members have paid their arrears making 152 names added to the roll since the New York volume was published. From the New York list, 24 names have been transferred to the list of Deceased Members, 20 Members have resigned and 100 have been omitted for arrearages, making a deduction of 144 from the list. Five have become Life Members; 36 members have been transferred to the list of Fellows, and one Fellow has been made an Honorary Fellow. Of the 1,964 now on the roll, 230 are at this date in arrears for the New York and Cleveland assessments.

The following gives a comparative statement of the roll as printed in the New York volume and in the present volume.

	New York.	Cleveland.
Patrons	3	3
Members	1285	1271
Honorary Fellow	1	1
Fellows	667	689
Total	1956	1964

Two hundred and fifteen papers were entered for the meeting, of which 33 were either declined or were not considered for lack of proper abstracts; 182 were put on the daily programme for reading in the sections as follows:— A 19, B 24, C 19, D 9, E 38, F 22, H 30, I 21. Of these 117 are printed in full or by abstract in the present volume, and 65 are mentioned by title. Of many of the latter, abstracts would have been printed had proper ones been prepared by the authors; others were to be printed in full in other places. In addition to the papers, the addresses of the President and seven Vice Presidents, and the reports of eight committees were given, and are printed in the volume. Three evening lectures were also delivered. Several of the papers were illustrated by lantern projections manipulated by the late Dr. Howland, who, at considerable trouble and expense, took his unsurpassed outfit of lantern apparatus from Washington to Cleveland.

The financial condition of the Association at the opening of the Cleveland meeting is shown by the annexed cash account, covering the year from August 1, 1887 to August 1, 1888, and including the New York meeting. From this account it will be seen that the last year was an exceptionally prosperous one financially, the receipts from regular sources, including admission fees, assessments and sales, amounting to $7,119.62; while the regular expenses of the year, including the publication and distribution of the New York volume, office expenses and salaries, were $4,952.14. In addition to the above receipts were the $1,723 in gifts which

more than paid the $1,602.78 expended for reprinting back volumes of the Proceedings so that full sets can now be furnished. Of the $2,751.45 due the Permanent Secretary for balances on salary for several years back, all but $463.70 was paid, which is the only debt of the Association.

$1,050 was obtained from life membership commutations and carried to the Research Fund. It will also be noticed that $300 income from this fund was granted for the encouragement of research at the New York meeting.

THE RESEARCH FUND amounted on Aug. 1, 1888, principal and interest to	$4,414.27
THE GENERAL FUND, Aug. 1, 1888, principal and interest, ...	$112.05

By vote of the Council, $200 was granted to aid in the archæological explorations at the Serpent Mound, by the Curator of the Peabody Museum.

Leaving total of invested funds	$4,326.32

The proposed formation of an American Geological Society and of an American Chemical Society has led to considerable discussion as to the good or bad effects of such organizations upon the respective sections in the Association of which they are the offspring. The discussion has also embraced the Entomological and Botanical Clubs of the Association, and the Society for the Promotion of Agricultural Science, which hold their meetings in connection with those of the Association.

Various opinions have been expressed which I do not propose to discuss, but there are some considerations worthy of the attention of all members of the Association.

In regard to the Society for the Promotion of Agricultural Science, it can be said without qualification that it has been of benefit to the Association. This Society has its annual meeting on the Tuesday preceding the first General Session of the Association, and, I believe, also one or two special meetings during the week, but always at times not conflicting with the sections with which the Society is affiliated. The result has been that most of the agricultural chemists of the country and many other men particularly interested in agricultural science have joined the Association and taken an active part in its meetings, while they would be drawn from it should their special society meet at some other time and place, and its members fully realize the importance of the benefits they receive as members of the Association, and the great opportunity they have of annually meeting so many workers in other but kindred branches of science.

The same is largely the case with the Entomological and Botanical Clubs, although in these clubs the members are primarily members of the Association and in regular attendance at the meeting. It can only be said against these clubs that many papers are presented and discussed in the clubs which would add largely to the interest of Section F; but it must also be acknowledged that the members of the clubs have been of late years among the principal supporters of the Section, and that the Section has certainly profited by the social elements of the clubs.

The result of the formation of the Geological and Chemical Societies can not yet be foretold, as that will depend upon the plan to be adopted for their meetings. Should these societies wisely arrange to hold their meetings so as not to conflict with the two sections which they represent, by holding their annual meeting on the Tuesday before the first General Session of the Association and their special meetings by agreement with the respective section to suspend its meeting for a half day or a day as might be arranged, and the rest of the time meet with the sections, it seems as if only good could result; particularly if all the members of the societies were also members of the Association, and this would follow naturally under such an arrangement.

Certainly in such a body as the Association, composed of workers in, and supporters and lovers of all departments of science, there is strength in union; and while any small portion might flourish for a time as an organization of specialists, entirely independent of the larger body of workers and supporters, it would be very short-sighted policy for any such body to cut the strings by which their own special work is bound to the still larger and grander work of the general advancement of human knowledge. With the mutual benefits to be derived from a close union of all special scientific societies — each necessarily narrowed by its specialty — with the great popular and spreading influence of the Association, where all united make a grand whole worthy the support of every one, it can hardly be questioned but that the mother of all will be faithfully sustained in the councils of her wise children.

F. W. PUTNAM,
Permanent Secretary.

Salem, Mass., May 9, 1889.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1887–8.

To admission fees New York Meeting	$1,825 00	
" " previous to New York Meeting	10 00	
Fellowship fees	110 00	
		$1,445 00
Assessments previous to New York . . .	819 00	
" for New York Meeting . . .	3,705 00	
" " Cleveland Meeting . . .	726 00	
" Associates, New York . . .	276 00	
		5,526 00
Publications sold	78 53	
Received for binding	63 14	
Postage and express refunded	3 13	
Incidental receipts	3 82	
		148 62
Gift of Mr. William Thaw of Pittsburg, Pa.	100 00	
" " " C. G. Hussey " " "	100 00	
" " " Geo. Westinghouse, jr. of Pittsburg, Pa.	50 00	
" " " Henry Phipps of Pittsburg, Pa.	50 00	
" " Ladies Committee, N. Y. Meeting .	303 00	
		603 00
Life membership commutations		1,050 00
Income Research fund		300 00
Reprint fund:—		
Mrs. Thompson's gift . . .	1,000 00	
3 years interest	120 00	
		1,120 00
Balance due Permanent Secretary, to next acct.		463 70
		$10,656 32

I have examined the above account and it

SALEM, MASS., AUGUST 7, 1888.

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

1887–8.		
By 2500 copies Proceedings Vol. 36 (466 pages)		
Composition and authors' changes . . .	$ 815 16	
Paper and press-work	600 00	
Printing paper covers and binding 2400 copies	264 00	
		$1,679 16
Binding 25 copies Vol 36, one-half morocco	25 00	
" 75 " " " cloth	37 50	
Cloth covers, 25	5 00	
Printing wrappers and wrapping 2500 vols.	34 50	
2850 extra copies of addresses and reports .	125 47	
		227 47
600 copies Constitution, List of Members, etc.	45 00	
Expenses of New York Meeting	188 51	
		233 51
Express, including distribution of Vol. 36 .	350 93	
Postage, Box rent, Postal Guide	201 81	
Telegraph and Telephone	2 84	555 58
Proceedings bought		12 00
Printing circulars, blanks, tickets, cards, etc.	213 20	
Petty expenses	8 54	
Extra labor	4 35	
Boxes for publications	12 35	
Rent of office	108 00	
Janitor to Aug 1	50 00	
		396 44
Memorial to President and Congress . . .	5 00	
T. C. Mendenhall, expenses to Washington	20 00	
		25 00
Expenses for Cleveland Meeting	69 00	
Advance copies Report for Cleveland Meeting	3 98	
		72 98
Reprinting 500 copies Vol. 22, Proceedings	1,400 08	
" 100 " Vol. 26, Proceedings (from plates)	117 40	
Illustrations for reprint of Vol. 26, balance in full	85 25	
		1,602 73
Grant to Profs. Michelson and Morley from Research Fund	175 00	
Grant to Dr. Minot from Research Fund .	125 00	
		300 00
Salary of Asst. Secretary to Aug. 1, 1888 .	500 00	
" " Permanent " " " " " .	1,250 00	
		1,750 00
Carried to Life Membership Fund		1,050 00
Balance due Permanent Secretary, 1886–7 acct.		2,751 45
		$10,656 32

is correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

INDEX.

ERRATA.

The paper by Mr. Horton on page 375 was printed before return of proof from author. Hence several minor errors have passed uncorrected. The only important errors noticed by the author are the omission of an "s" after "economist," page 378, line 15; and the word "in" for "is" in line 25 of the same page. "Trade is Barter."